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CONTENTS

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This directory gives biographical data and fields of interest for the Fellows and Associates of the American Psychological Association. Membership lists for the Divisions of the Association, the by-laws, a list of past officers and meeting places, and a geographical and institutional index of members are included. It is edited by Helen M. Wolfle of the Association staff.

According to present plans of the Association, another directory including biographical data will not be published until 1951. The interim issues will contain the names of the members, their addresses, and their present positions.

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Psychological Bulletin

THE PREVOST-FECHNER-BENHAM SUBJECTIVE COLORS

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Color vision, of all psychophysical phenomena, has perhaps the most elegant scientific formulation. The color manifold can be represented as a closed three dimensional space with reference to three arbitrary primaries. It thus fulfills the space requirements of solid analytical geometry and all transformation formulas referring to vectors in space can be made to apply.[1]

If the intensity function is eliminated by projecting the $x+y+z=1$ plane onto the xy plane, then the resulting configuration, called a chromaticity diagram, fulfills all requirements so that all theorems of projective geometry for closed spaces can be made to apply. We do not believe that there is any other psychological or biological function which may be so adequately described by mathematics so concise.

This remarkable fact has been noted by the mathematician, H. Weyl. With respect to the isomorphism between the projective plane and the sensation, he adds (83, p. 18),

> . . . . the projective plane and the color continuum are isomorphic with one another. Every theorem which is correct in one system . . . is transferred unchanged to the other . . . A science can never determine its subject matter except up to an isomorphic representation. The idea of isomorphism indicates the self-understood, insurmountable barrier of knowledge. It follows that toward the 'nature' of its objects science maintains complete indifference . . . For example, what distinguishes the colors from the points of the projective plane—one can only know in immediate intuition.

[1] This is actually the basis of modern color theory, but in recent years some workers have found it necessary to question the Euclidean nature of the space. This is stated implicitly by Troland (79) and explicitly by Silberstein (69, 70). This latter series of papers presents a remarkable insight into what is involved in the mathematical considerations of the color domain. Silberstein asks whether the space is *Desarguesian*, and presents a striking technique whereby this concept may be put to empirical test.

The experimental fact that all colors may be reproduced by three real primaries involving negative coefficients, or three imaginary primaries involving only positive coefficients, leads to the inevitable hypothesis that there are three receptors in the retina, each of which is associated with a so-called "fundamental sensation curve." The enigma, the great enigma, of color vision is that *any* three points (not on a straight line) will serve as primaries, since lines connecting them will completely span the projective or chromaticity plane. Mathematically, any three points, corresponding to the primary colors, will produce a unique set of fundamental sensation curves, and are therefore as good as any other set of three points. If the object of science is prediction, then any set will predict color mixture data as well as any other set. However, if a choice of primaries can be made so that they predict something more than just the phenomena of color mixture, then they are desirable from the point of view of parsimony. People have, of course, done just this. Helmholtz selected his primaries on the basis of the fact that there are no changes in hue from 670 millimicrons on out to the long wave end of the spectrum, Hering on the basis of after images, and Ladd-Franklin on a phylogenetic basis. For a fine discussion on the nature and transformation of these primaries, see Judd (**48**).

There still remains an outside criterion for the selection of the primaries which has received little consideration from workers in color. We refer to the phenomenon of subjective colors, first discovered by Bénédict Prevost 122 years ago. We propose to present here a detailed history of the experimental work on these colors, followed by a brief discussion of the logic by which these colors may be made to determine the primary receptors and the corresponding projection and fundamental sensation curves.

The history of the phenomenon divides itself into several natural divisions, and the analysis will be in terms of these divisions.

I. Prevost's Discovery and the First Period of Observation

Bénédict Prevost (**62**), a French monk, discovered the subjective colors in 1826. He observed a ray of white light in a darkened chamber. He moved a white, rectangular cardboard perpendicularly to the ray, such that the ray was cut at successive intervals. He noted that the ray was white, at first, then a violet in the center, then a deep indigo, then a greenish-yellow followed by red. "One sees," he writes, "that the light of the ray . . . has been decomposed as if by a prism, into the seven principal colors ranging in almost the same order."

This is not to say that Prevost believed that he had actually de-

composed the light as Newton did. On the contrary, he was aware that the effect was physiological in nature, and proposed a theory which has some degree of merit. Presumably, there are substances which combine with the nerves in the retina, giving rise to the sensations of color. These substances act at *different rates*. Since white light is composed of different wave-lengths, intermittent white light should produce the subjective colors seen. Since the concept of three receptors was unknown to Prevost, his theory compares most favorably with those of later more sophisticated investigators.

The first rediscovery of the subjective colors was made by G. T. Fechner (33) in 1838. It is Fechner who is usually credited with the original discovery. It was well known to Fechner that a disk whose composition is partially black and partially white will produce a gray when rotated, the luminosity of the gray being a function of the ratio of black to white. He noted, too, that the disk must rotate above the fusion frequency.

In order to produce various gradations of gray, Fechner prepared a disk which was divided into eighteen concentric circles, the diameter of each circle differing from the adjacent circle by a unit length. The innermost circle was completely black, the outermost circle was completely white. The remaining circles varied in the relative amounts of black and white as illustrated in Fig. 1a. The disk included in the Fechner paper shows only seven concentric circles. This was done so that the diagram might be clear, but Fechner never used such a disk. It is erroneously attributed to Fechner by many later investigators, for example, Colenbrander (24)

The circumference of the black figure (disregarding the corners) forms an Archimedes' spiral. While trying to spin the disk at a speed above the fusion frequency, Fechner spun it below the fusion frequency, and was amazed to see the many and varied colors which were produced. The colors, he notes, were not saturated, and yet were unmistakable. Other persons saw them in various degrees of distinctness. Some called the colors brilliant, some saw them only indistinctly, but all agreed that some color was evident.

Now Fechner begins to experiment with the possible determiners of subjective color. Speed of rotation and direction of rotation are factors. If white partial circles are used on a black background, as in Fig. 1b, colors are again produced although not in the same position as before.

If a disk of the type shown in Fig. 1c is rotated at high speed, a gray is produced as expected. At moderate rotation the disk color is yellow; at successively increasing speeds, it passes through yellowish-green,

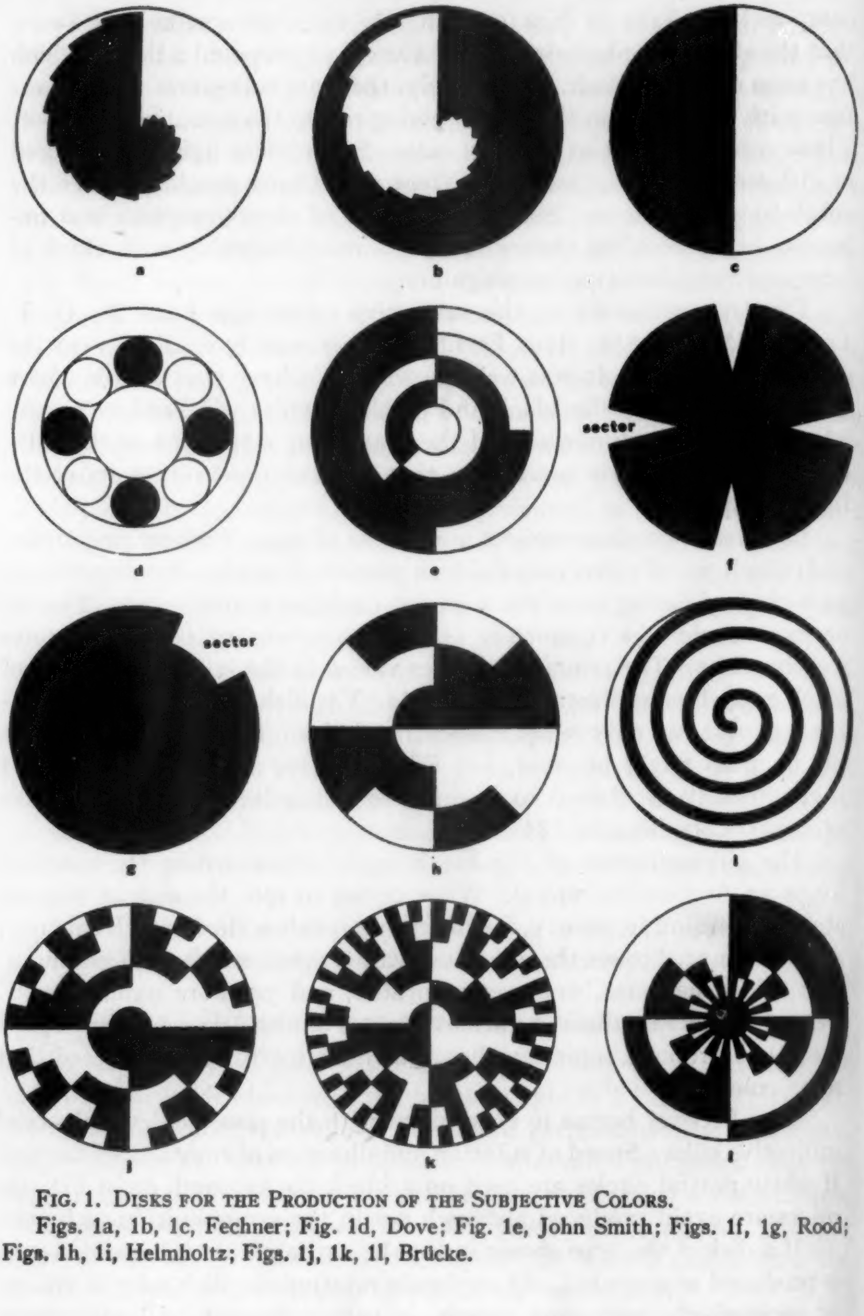

FIG. 1. DISKS FOR THE PRODUCTION OF THE SUBJECTIVE COLORS.

Figs. 1a, 1b, 1c, Fechner; Fig. 1d, Dove; Fig. 1e, John Smith; Figs. 1f, 1g, Rood; Figs. 1h, 1i, Helmholtz; Figs. 1j, 1k, 1l, Brücke.

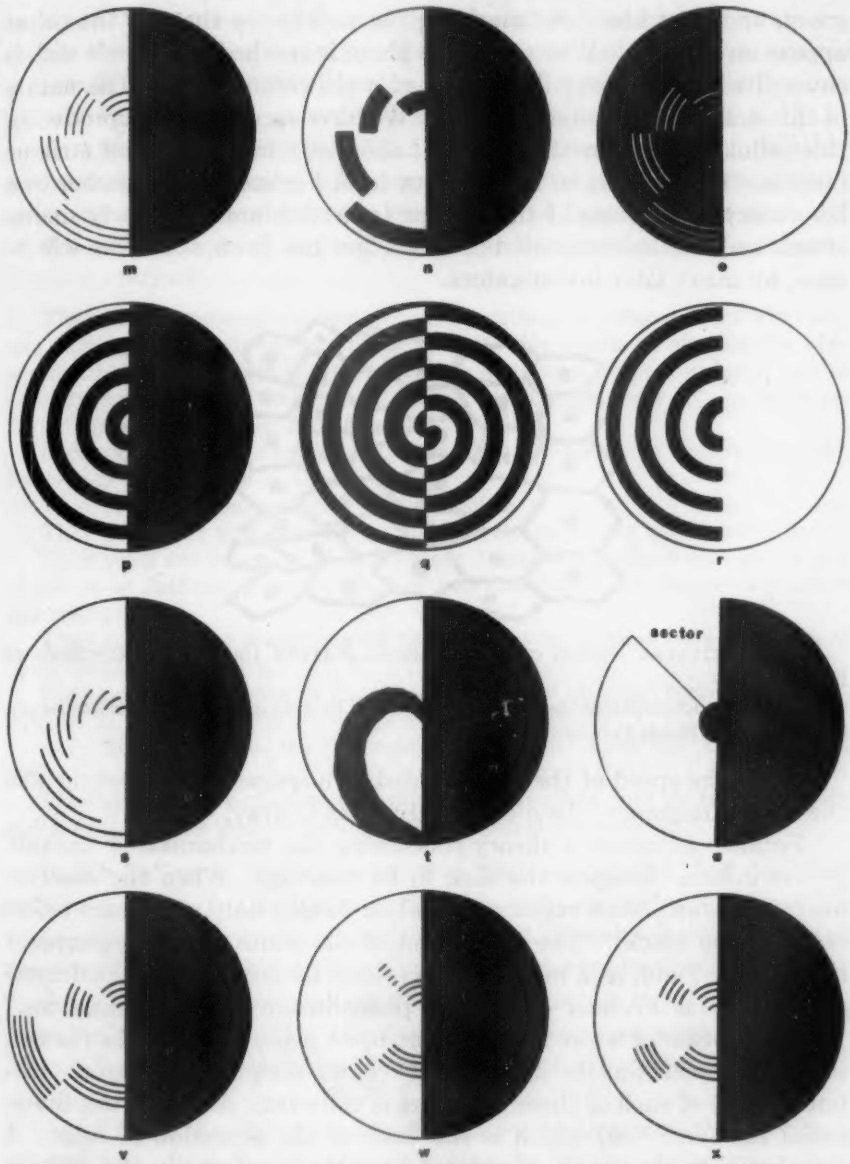

Fig. 1. (continued). Disks for the Production of the Subjective Colors.
Fig. 1m, Benham; Figs. 1n, 1o, Finnegan and Moore; Figs. 1p, 1q, 1r, Hurst; Figs. 1s, 1t, Wolf; Fig. 1u, Bidwell; Figs. 1v, 1w, 1x, Bagley.

green, and light blue. A "marbling" is interwoven through the colors appear on the disk. When the blue phase is reached, the whole disk is covered with a tapestry-like design of multi-colored cells. The nature of this design is suggested in Fig. 2. We have succeeded in reproducing this cellular design in the Illinois Laboratory in a clear and striking fashion. The drawing in Fig. 2 is not from Fechner, but from our own laboratory. The bases of the cellular formation are not clearly understood, and the existence of the formation has been noted, as will be seen, by many later investigators.

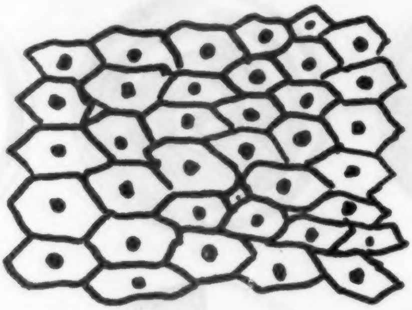

FIG. 2. CELLULAR DESIGN OF A HEXAGONAL NATURE ORIGINALLY REPORTED BY FECHNER.

The design accompanies the subjective colors. The drawing is from experiments performed at the Illinois Laboratory.

When the speed of the rotating disk is increased ever-so-slightly to the fusion frequency, the disk quickly fuses to gray.

Fechner advances a theory concerning the mechanism of the subjective colors. Imagine the disk to be rotating. When the observer fixates the disk, black replaces the white (which only an instant before replaced the black). The impression of the white does not disappear immediately, and, as a matter of fact, lasts for some time as is demonstrated by, as Fechner says, "the phenomenon of after sensations." The perception of white is dependent upon many receptors in the eye, probably three. But the rate of fall (Fechner does not mention the rise, but should) of each of these receptors is different. And it is this differential fall (and rise) which is the basis of the sensation of color. A moment later the eye is stimulated by white light again, the white is replaced by black, and the entire cycle repeats itself.

Dove (26, 27) in 1848 noted the relation between Fechner's colors and those produced as a simple after sensation. He describes disks similar to those of Fechner, with the difference that white is replaced by another color. When the disks are rotated he describes the resulting per-

ception. The paper is of theoretical value since it demonstrates the relation between after sensations and subjective color—that all colors are really subjective—but makes no really significant contribution. Dove's disk is reproduced, after his description, in Fig. 1d.

In 1859 John Smith (73, 74) made *the second rediscovery.* He believed that the subjective colors were a direct result of the physical characteristics of light and that the new theory of light which he proposed would replace that of Newton. We reproduce verbatim a portion of Smith's abstract.

> The author produced the same results by cutting out spaces in a white card, and causing it to revolve on a black surface. He produced also similar phenomena by causing these figures to revolve when held perpendicularly, and to take the appearance of coloured solids. He also caused these colours to be reflected on a white surface from the revolving disk.
>
> Remarkable as these experiments are, they are not more remarkable than the results they lead to.
>
> They prove the homogeneity of the ether.
>
> They prove the undulatory hypothesis, but oppose the undulatory theory.
>
> They show the necessity of introducing a negative element into the theory of colour, or that colour is the effect of two coordinate sensations—a positive and negative.
>
> They enable us to dispense with the different refrangibilities of the rays of light, taught by Newton.
>
> They remove the necessity for the supposition of different waves or of a disposition in matter to produce waves of different lengths.
>
> They help to explain the phenomena of what is called the polarization of light.
>
> They give a new explanation of prismatic refraction, and explain in a plain and simple manner many very interesting natural phenomena.

Smith indicates that there are five rings on his disk, but describes only four. Fig. 1e illustrates J. Smith's disk, reconstructed from his description. One may conclude that Smith had perhaps misinterpreted the theoretical aspects of his rediscovery of the subjective colors.

Rood (63) in 1860, realized that the theory of light proposed by John Smith was invalid, since the color appearances were subjective. He confirmed the existence of the colors, but disagreed as to their origin.

Rood first observed the subjective colors while attempting to measure the time of small explosions of gunpowder by use of a rotating disk. For this purpose he used a black disk with four radial slits, reproduced in Fig. 1f. A certain speed caused the lower part of the flame to appear green, while at slower speeds the entire flame appeared to be a purplish-red. At very slow speeds of rotation, the appearance of the flame alternated between white and violet.

A more elaborate disk of the type illustrated in Fig. 1g was now constructed. It was nine inches in diameter, while the slit was one half inch at the circumference. The disk was rotated against a white cloud. At a rotational speed of ten revolutions per second, the cloud appeared to be a deep red.

The disk was viewed through a colored filter whose transmission properties were such that it transmitted the red, orange, yellow, some green, but excluded the blue and violet. If radiations coming from the disk were red and only red, then the filter should transmit these rays and the resulting sensation should be red. The sensation, under these conditions, is not red but a greenish-blue, indicating the subjective nature of the phenomenon. The subjective nature is further shown in that if a yellow glass is employed, the disk appears violet; if a green glass be used, the disk appears green, neutral, or faint red.

Brewster (16) in 1861 made *the third rediscovery* of the subjective colors before becoming familiar with the work of Prevost and John Smith. He observed the colors while watching a rotating series of slits pulse a white light source. The colors, beginning at the center of his observational field, were as follows: white or bluish, darker blue, white, a well defined dark ring, white, greenish yellow, reddish. He suggests that similar phenomena may be seen in a flickering gas flame.

Using an apparatus consisting of a rotating disk in front of an illuminated ground glass, he was able to reproduce the cellular, hexagonal pattern reported by Fechner. He reports, too, an additional quadrangular ". . . pattern, which is too evanescent to permit it to be drawn, [which] consists of a series of dark quadrangular spaces separated by triple or multiple lines of light. It has no relation whatever to the hexagonal pattern, and has never been seen unless the eye has been strongly impressed by the successive impulses of the luminous disk." He suggests that the hexagonal forms may represent cells in the choroid coat; he is uncertain concerning the origin of the quadrangular forms.

Rood (64) published a short note in 1861 in which he reviewed the meager literature on subjective color then available. He suggested that the hexagonal pattern reported by Brewster might be related to similar observations reported by Purkinje and Vierordt (see **43**, p. 257).

II. Helmholtz to Stewart—the Scientific Formulation

The genius of Helmholtz began the second well defined historical period. Here we find the first formal applications of scientific principles. Helmholtz (**43**) in 1860 (translated in 1924) pointed out that if an observer fixates a Maxwell disk with black and white sectors, he notices

that the white is red on the advancing edge and bluish on the other edge. In reduced illumination, the red becomes reddish yellow, and the blue becomes violet. In intense illumination, the red becomes pinker, and the blue becomes greenish. "With slower rotation," he writes, "the bluish hue at first is spread over a wider part of the white than the reddish. But when the speed is faster, red spreads out all over the white as pink-red, and green-blue extends over into the black sectors; and on the whole, violet appears to predominate on the disk." These three phases can be demonstrated simultaneously by use of a disk reproduced in Fig. 1h. The colors can be seen somewhat more easily by use of the disk in Fig. 1i.

Practice is necessary to see the colors in their maximum saturation. Apparently, the eye must be fatigued by the flicker to arrive at the optimum conditions. He reports the hexagonal cells. If a partially colored and partially black disk is rotated, the observed colors tend to fade and be non-homogeneous. He believes that the "fluttering heart" illusion is related to the subjective colors.

Helmholtz' theory is that of Fechner—that the three receptors have a differential rise and fall. The blue, says Helmholtz, lags behind the red.

Brücke (19) utilized disks illustrated in Figs. 1j, 1k, and 1l. He proposed to show that Talbot's Law, as a result of Fechner's experiments in subjective color, was not true under all conditions. Following a theoretical discussion of the relation between the objective stimulus and the resulting sensation, he spun his disks in order that he determine which speed produced the brightest appearance. The brightest color, obtained when violet just about goes over to blue, was observed at an average rate of 17.6 white pulses per second.

He concluded that the perception of the subjective colors was closely allied to what modern investigators call adaptation or fatigue, *i.e.*, the perception brought about by the steady stimulation of the retina. Actually, by definition, the subjective colors are produced only by intermittent light. For example, after images, although really as subjective as any other color experience, are not subjective in this sense. The entire discussion is confused by the interchange of what we would call the adaptation or fatigue of the receptor and the fall of the receptor.

Aubert (4) included a discussion of the subjective colors in his famous book on physiological optics in 1865. He indicates that he sees the colors on Brücke's disk at the greatest saturation after a fixation of about one half minute. The speed of the disk is a determiner of the colors seen. If the illumination of the disk is decreased, then the speed

of rotation of the disk must be increased in order that the appearance remain constant.

With reference to the Brücke disk (Fig. 1j), he says that if the frequency of the white pulses is more than 56 per second, then the ring appears homogeneously gray. If the pulses are between 42 and 48 per second, and an outside part of the ring is fixated, then the part in direct vision will be gray while the part in indirect vision will appear to be a Berlin blue. As the speed decreases the extension of gray in direct vision decreases. The ring does not appear entirely uniform, being interposed with dark and light radial stripes, and blue and gold become apparent. As the speed is decreased the blue and gold become more intense and the square (hexagonal?) cell structures are seen. As the speed of the disk continues to decrease the extent of the blue field decreases until it disappears at 10 pulses per second. Various other color phenomena are described.

Exner (32) in 1870 also observed that the subjective colors appear to differ when viewed in the center or the periphery of the eye. His introspective descriptions are given in great detail. The colors, he points out, are seen in greater saturations when the eye is fatigued. But the fatigue of one eye does not facilitate the perception of subjective colors in the other eye, and it would follow, therefore, that this aspect of the subjective colors is not a central effect.

Exner reported the presence of quadratic or six sided pointed elements, and expressed his belief that an explanation of them would be of fundamental importance to physiological optics.

F. J. Smith (72) made *the fourth rediscovery* in 1881. He theorized that light might be decomposed by interruptions proportional to the wave-length of a particular ray of the many, which together are the composite ray. He used a large bicycle wheel between the sun as a light source and the observer, and produced the subjective colors. Performing simple experiments he believed that he had derived the wavelengths of the spectral colors.

The fifth rediscovery was made by Hannay (42) in 1881. While working on dry photographic plates in a room illuminated by a ruby light, he noticed that movements of his fingers produced a greenish blue, with blue predominating. He independently derived the Fechnerian theory of the rise and fall of the receptors, and suggested that this phenomenon might be used as a basis for determining the fundamental color sensations.

N. Smith (75) stated in 1882 that Hannay's explanation (the Fechnerian theory) was suitable for colors seen in the photographic dark-

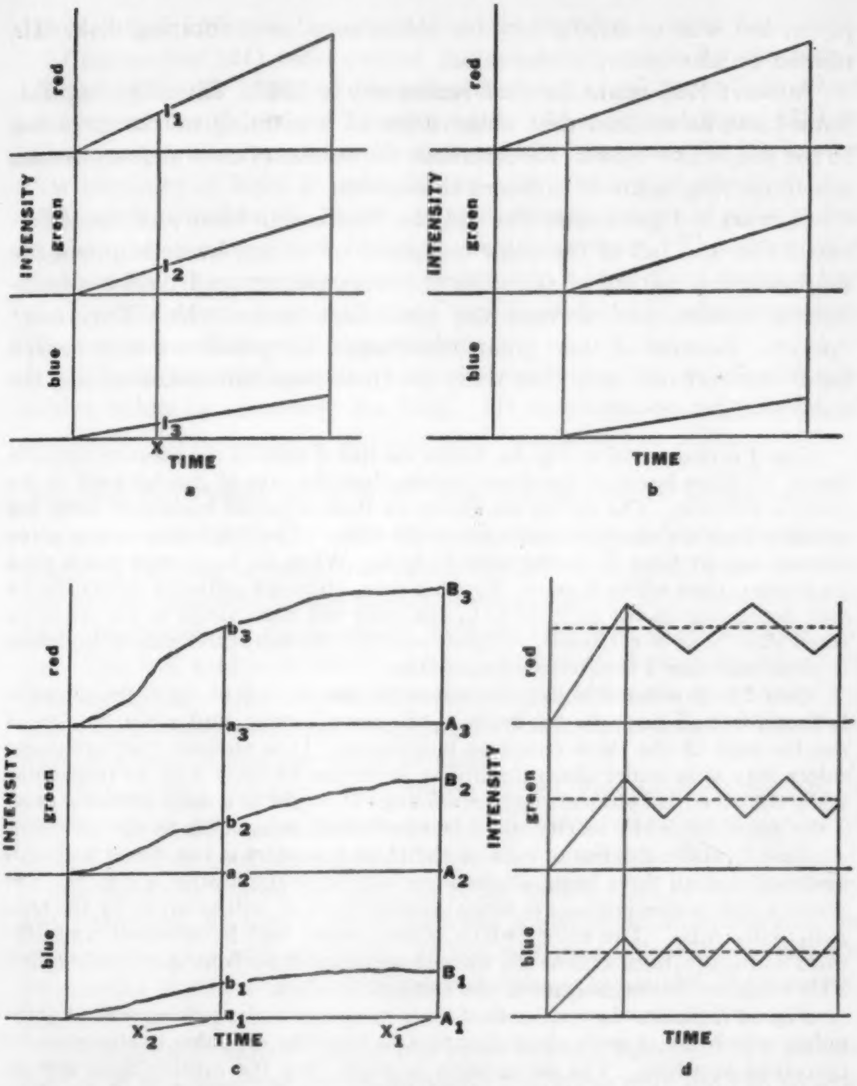

FIG. 3. Stewart's ANALYSIS OF THE FECHNERIAN THEORY OF THE DIFFERENTIAL RISE AND FALL OF THE THREE RECEPTORS.

Fig. 3a illustrates the case where the rise is linear, the slopes are different, and all three receptors begin the rise at the same time. Fig. 3b illustrates the case where the rise is linear, the slopes are different, and the three receptors begin their rises at different times. Fig. 3c illustrates the case where the rise is non-linear, the slopes are different, and all three receptors begin the rise at the same time. This last case is shown to be the most adequate. Fig. 3d indicates the responses of the three receptors under pulsing white light—pulses which are of such short duration as to be rarely perceived as white.

room, but was unsatisfactory for colors seen on a rotating disk. He offered no alternative explanation.

Stewart (76) made *the sixth rediscovery* in 1887. Investigating Talbot's Law, he noticed that observation of a rotating mirror gave rise to the subjective colors. He described the results of careful observations under varying lights of different intensities.

Stewart independently derived the Fechnerian theory of the differential rise and fall of the color receptors. Further, he determined the mathematical nature of the different response curves from his experimental results, and derived the conditions under which they must operate. Because of their great importance, his proofs are summarized here. Stewart indicates that there are three possibilities concerning the nature of the curves:

Case 1 is illustrated in Fig. 3a, where the rise of each of the three receptors is linear, all three begin at the same instant, but the rate of rise for each of the three is different. The curves are shown on three separate horizontal axes, but actually they are superimposed one on the other. The color seen at any given instant, say at time X, is the ratio $I_1:I_2:I_3$. When I_1, I_2, I_3 each reach their maximum, then white is seen. For this case, although different colors can be seen depending on the ratio $I_1:I_2:I_3$, the color will not change as the receptors reach their peak of excitation. Experimentally, the subjective colors do change in time, and case 1 is therefore impossible.

Case 2 is illustrated in Fig. 3b, where the rise of each of the three receptors is linear, but all three do not begin at the same instant, and again the rate of rise for each of the three receptors is different. It is obvious that subjective colors may arise under these conditions, since one receptor may be responding while the other receptors are not responding. It would be a more powerful proof if the assumption of linearity could be eliminated, bringing us to the next case.

Case 3, where the rise of each of the three receptors is non-linear and, it is assumed that all three begin at the same instant, is illustrated in Fig. 3c. The color (which in this instance is white) seen at time X_1 will be given by the ratio $A_1B_1:A_2B_2:A_3B_3$. The color (which is not white) will be different when the ratio is $a_1b_1:a_2b_2:a_3b_3$ at time X_2, since these ratios differ from the former ratios. This explains the mechanism of the subjective colors.

Fig. 3d indicates the responses of three receptors under pulsing white light—pulses which are of such short duration so that the stimulus is almost never perceived as white. The assumption is made that the pulsing light sets up different phase relationships among the receptors, since the natural periods of each of the receptors is probably different. Observations indicate that as the intensity of the pulsing white light is increased, the subjective colors tend toward violet. The explanation is found with reference to Fig. 3d, where it is assumed that the change in rate of rise is most appreciable in those curves which, at a given speed, are less steep.

Stewart reports many experiments in which colors (some monochromatic) other than white were pulsed. He reports many subjective

colors, which indicates that more than one receptor is being stimulated.

Charpentier (**21**) rediscovered the subjective colors for *the seventh time* in 1891. He observed the phenomenon by looking at light which was intermittently pulsed through holes. In a second paper (**22**) he discussed the theoretical implications of his "discovery." He reports that intensity of light is not a factor. He states that the subjective colors tend to refute the Helmholtz trichromatic theory of color vision, because, all colors are reproduced, not just three. The alternative theory which he proposes is not reasonable. In a third paper Charpentier (**23**) describes an additional technique for producing the colors. The paper is not of great importance.

Sanford (**65**) was apparently the first individual to include the subjective colors in classroom teaching. He discussed an experiment on the colors in his course in physiological psychology in 1893.

III. Rediscoveries in 1894–95. The Reversion to Observation

In the years 1894 and 1895 there began in England a period of considerable interest in what was called the artificial spectrum top. The discussions appeared in the British journal *Nature*, and were keynoted by a description of the so-called Benham top (**84**), illustrated in Fig. 1m. This top, sold extensively as a toy, presents colors in an extremely striking fashion—and is even more interesting since the position of the colors is reversed when the direction of rotation is reversed. The Benham disk constituted *the eighth rediscovery*.

Liveing (**52**) presented an address to the Philosophical Society of Cambridge in 1894 in which he presented a variety of disks similar to that of Benham and presented a theory very similar to that of Fechner.

Benham (**9**) joined the discussion by saying that Liveing's theory was wrong and his own correct, since the colors could be seen under the illumination of a sodium lamp. It was not made clear as to why the Fechnerian theory would not account for the colors under these conditions.

Liveing (**53**) replied that he had observed the top under the sodium light, and had observed colors, although the colors were not the same as those observed under white light. He too fails to state that the Fechnerian theory will account for the colors, but merely repeats that the colors are of a subjective nature.

Abney (**2**) rotated the Benham top in white and near-monochromatic lights. The following colors were observed, where No. 1, No. 2, No. 3, and No. 4 are the triple lines of the top (see Fig. 1m) in order from the center out:

With white light: No. 1 crimson, No. 2 olive green, No. 3 gray (slightly violet), No. 4 dark violet; *with red* (*C light*): No. 1 red, No. 2 lighter red, No. 3 very light olive green, No. 4 darker olive green; *with green* (*Magnesium b*): No. 1 bluish green, No. 2 lighter bluish green, No. 3 lighter bluish green, No. 4 ruddy black; *with blue* (*near the blue Lithium*): No. 1 grass green, No. 2 lighter grass green, No. 3 lighter grass green, No. 4 ruddy black; *with violet* (*all the violet of the spectrum*): No. 1 light violet, No. 2 light violet, No. 3 light violet. No. 4 darker violet with a suspicion of red.

Abney states, "When a red a little below the red lithium line was employed, all the groups appeared dark red, and, as in the three sensation theory this part and the violet are simple sensations, the results obtained in these last were to be expected."

Continuing with mixtures of two simple colors, the following results were obtained:

With a mixture of red and green to make white: No. 1 indigo blue, No. 2 reddish orange, No. 3 reddish orange, No. 4 darker orange; *with a mixture of yellow and blue to make white:* No. 1 sky blue, No. 2 sage green, No. 3 sage green, No. 4 bluish black (perhaps black).

These results are extremely important in the theoretical application of these colors to the isolation of the primary receptors.

Finnegan and Moore (**34**) constructed two disks illustrated in Figs. 1n and 1o. They found that the width of the lines was highly important. If the lines are too broad, then color appears only on the edge. Further, the sequence of black and white with respect to the lines and the rate of rotation are determiners of the colors seen.

Edridge-Green (**29**) pointed out that the subjective colors of the Benham top were previously discussed by Helmholtz and Fechner. He reports the hexagonal cell structure when the Benham top is rotated.

Benham (**10**) mentioned in a note that his theory (which is improbable) would adequately account for the change in colors under change in rate of rotation mentioned by Finnegan and Moore. The statement is not clear.

Turner (**80**) suggested an apparatus whereby a glass spectrum top is inserted in the slide holder of a projector, rotated and projected, so that demonstrations might be made to large audiences. He also proposed the use of colored filters for additional effects.

Newton and Co. (**55**) called the attention of the scientific public to the fact that the Benham disk was copyrighted and that the use of the disk for projection was prohibited.

Hurst (**45**) then suggested three new disks which were not copyrighted. They appear in Figs. 1p, 1q, and 1r.

A note appearing in the British journal, *Engineering* (**86**), made

mention of the Benham top. A theory of one MacFarlane Gray is discussed. According to Mr. Gray, the subjective colors are caused by the color aberration of the lens of the eye. He proposes an "eye-demon" which compensates the lens, under normal conditions, for every change in seen color. When compensation cannot be made quickly, subjective colors are seen.

These notes were followed by still another in the *Scientific American* (**85**). The Benham top is again presented. MacFarlane Gray's theory is discussed with the statement, "It does not, perhaps, present the degree of clearness and precision to which we are accustomed in the study of optics. The field remains open to investigation for varying the experiments and completing the first exposed." Two new disks, designed by Charles E. Wolf, were presented and are illustrated in Figs. 1s and 1t. The disk in Fig. 1s is the obvious extension of the Benham disk, since the lines will produce finer gradations of color. Fig. 1t is the same as Fig. 1s, with the difference that the lines have been connected so as to produce continuous variations in color.

James McKeen Cattell (**20**) concluded the 1895 series of notes by pointing out that the spectrum tops were introduced to modern science by Fechner, Brücke, and Helmholtz, and hence were not patentable.

Henry (**44**) in 1896 attempted to explain the colors of the Benham top by the differential sensitivity of the periphery and fovea of the retina. He even went so far as to determine the different sensitivities of the foveas of several individuals. This is not the mechanism of the colors, as will soon be seen from the experiments of Doniselli and Piéron.

In 1896, Schwartze (**66**), in an appendix to his book on molecular physics, attempted to show that "the Newtonian hypothesis . . . is based on the absurd assumption of the multicoloration of the unicolored light," by use of Fechner's disk. This he did unsuccessfully.

Bidwell (**11**) in 1896 mentions several new methods of producing subjective colors. These are somewhat similar to those of Charpentier, to whom Bidwell makes reference. Bidwell believes that the colors of the Benham top may be accounted for by his theory that color fibres adjacent to those of the stimulated area of the retina continue to respond after a stimulus has ceased, by a process of "sympathetic reaction."

In a second paper Bidwell (**12**) in 1897 stated that the greenish blue subjective color observed in his previous experiments was due to the after image of the subjective red color. This is rather remarkable for, as Bidwell himself states, the greenish blue may be much more intense than the red. He made the additional observation that the illumination

is a determiner of the color seen. He found, further, that an observer may not be able to distinguish between a real and a subjective red. In the following year, Bidwell (13) reviewed his experiments and published a picture of his disk which appears in Fig. 1u.

A note then appeared in the French journal *La Nature* (41). The colors were demonstrated by means of a half black, half white disk, with a triangular cut out (similar to Bidwell, see Fig. 1u). When rotated in front of a red field, one sees blue-green instead of red. The theory is advanced that the colors are due to retinal fatigue.

IV. Bagley and Doniselli. The Psychophysical Approach

In 1902, Bagley (5) began a new period in the history of subjective studies with the first, and one of the best, systematic investigations of the phenomenon. Included with this study is the first extensive historical survey.

Miss Bagley utilized ninety-seven modifications of the Benham disk, three of which are illustrated in Figs. 1v, 1w, and 1x. Systematically, Bagley studied the effects of the length of lines on the color seen, the effect of the width of the line on the color seen, the effect of the distribution of the groups of lines on the disk, the effect of background color, the effect of the amount and distribution of background color.

Spinning an ordinary Benham disk, she reports that black followed immediately by white shows the lines to be red. When black is both followed and preceded by white, the lines appear to be green. Variations of the amounts of white vary the color of the green. Black preceded by white shows the lines to be blue. When lines which appear to be red green, and blue, under normal rotation are lengthened, the colors appear to be less intense but not less saturated. As a general rule, the width of the lines has no effect on the hue seen, but it may effect the other color variables.

When the black sector is varied, experiments show that the colors seen are not dependent merely upon position, but upon actual duration of the stimulation. With an increase of black, the reds are emphasized. When white is increased, the blues are emphasized. In bright daylight, the blue is emphasized. In less intense artificial light, the reds are emphasized.

Reversal of rotation of the Benham disk does not give an exact reversal of the colors seen. The hues are reversed, but the colors appear in different saturations and intensities.

When a gray screen is placed about a particular ring of one of the disks, the effect is to decrease the color seen of the ring. When the white

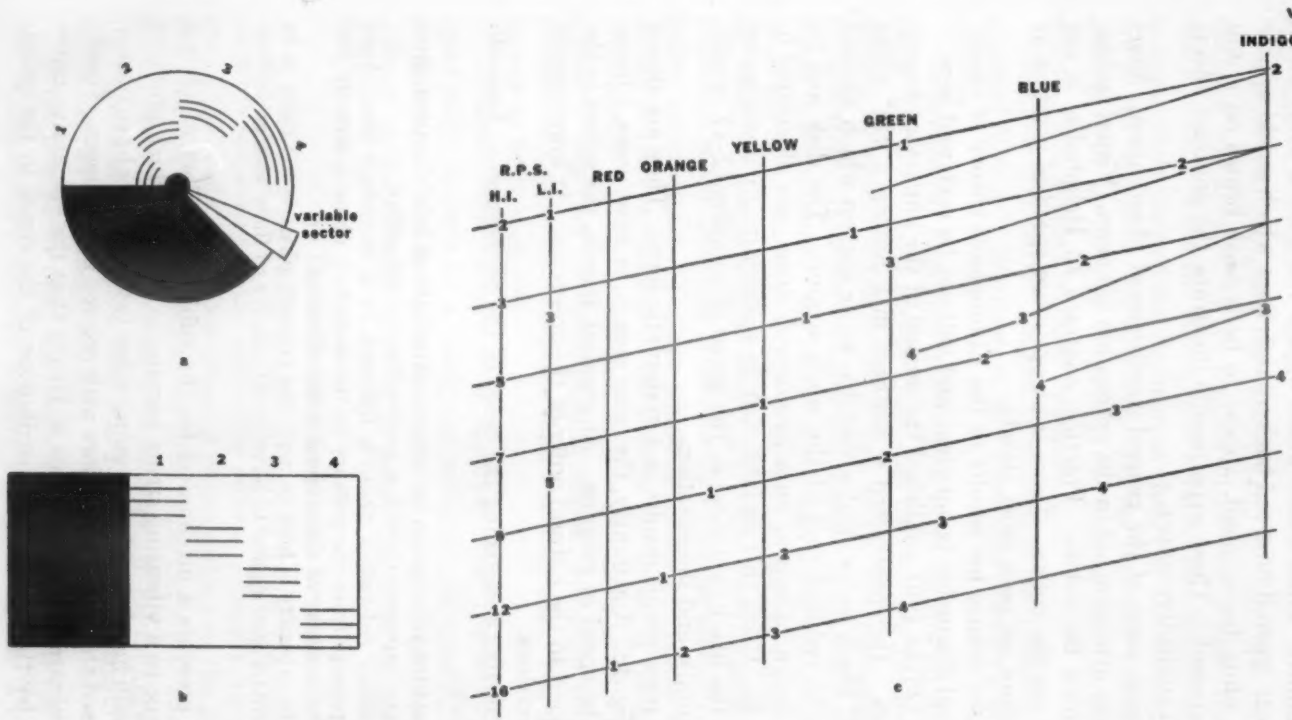

FIG. 4. DONISELLI'S ANALYSIS OF THE SUBJECTIVE COLORS.

FIG. 4a illustrates a Benham top which has a variable black sector. Fig. 4b is the analogue of Fig. 4a—a design to be used on a kymograph drum. Fig. 4c is a redrawing of Doniselli's results. The numbers refer to the arcs in Figs. 4a and 4b. The colors seen on the arcs are shown to be a function of the speed of rotation and the level of illumination. R.P.S. = Rotations Per Second. H.I. = High Intensity. L.I. = Low Intensity.

of the Benham disk is replaced by a physical color, very interesting results are obtained. Lines which would appear red if on a white background, appear green if on a red background; lines which would appear blue if on a white background, appear to be reddish brown on a dull orange background. These experiments illustrate the contrast effects possible with subjective colors.

Bagley notes some of the general conditions of observation. Short periods of keen attention aid in the perception of colors. Visual fatigue tends to deaden the colors. Practice, contrary to Helmholtz, is not necessary to see the colors. There is an optimum speed of rotation at which the colors are seen most clearly.

Miss Bagley relates her results to the Ebbinghaus theory of vision. Her theoretical discussion is not clear and will not be reviewed here.

Doniselli (25) in 1907 published the second of the important experimental papers. He constructed a Benham disk having a 225° white sector, a 135° black sector, and a variable white section which allowed the black to be reduced to $\frac{1}{3}$ of the white section. The disk and its analogue, a design to appear on a kymograph drum, are illustrated in Figs. 4a and 4b. Using this variable disk he systematically investigated the effect of the black/white ratio, the speed of rotation (2–17 revolutions per second), and illumination.

Doniselli presents his results in a systematic form. They are shown redrawn in Fig. 4c. Apparently, the color seen is, in most cases, a linear function of the speed of rotation. At greatest speeds, the colors in the rings correspond to the natural order of the spectrum. At lower speeds there are inversions.

In his theorizing concerning the cause of the phenomenon, Doniselli writes as follow:

a. Every sudden stimulation by intense white light of brief duration, gives rise to a primary impression which is predominantly red-yellow.

b. The primary red-yellow phase is followed by a secondary blue phase. The speed of passage from the primary to the secondary phase is directly proportional to the intensity of the original white stimulus.

c. When the intensity is high enough, the primary phase is too rapid to be seen, and the colors seen appear to be reversed relative to those seen at medium intensities.

Doniselli presents a mechanism for the subjective colors which is a direct analogue to a vibrating string varying in tension, which produces tones. Doniselli has done what so many other investigators have done—he has confused the subjective colors with colored after images. Finally Doniselli eliminated the hypothesis of Henry that the subjective colors were caused by the differential distribution of the cones in the retina,

by demonstrating the colors on a sheet of paper rolled about a cylinder (Fig. 4b).

Sexton (68) discussed the Benham top in 1907. He indicated that the color was affected by the position of the sector lines, the length and width of these lines, the direction of rotation and the illumination. He independently derived the Fechnerian theory of the differential rise and fall of the receptors. He observed that the fact that colors are seen in sodium light in no way disproves the Fechnerian theory—since the sodium light affects the three receptors.

Percival (57) in 1909 became interested in the problem, but simply repeated Stewart's experiments and presented Stewart's diagrams and theories.

Baumann (6) published the first of three papers on the subject in 1912. He described a simple apparatus for rotating the disks, and described the colors of the Benham disk which he erroneously attributes to Schwartze. He presents three new disks. In a second paper (7), he states that the smaller the image of the disk upon the retina, the more vivid are the subjective colors. In his last paper (8), published in 1918, he presented five new disks which are minor variations on Wolf's tops published in the *Scientific American* (see Figs. 1s and 1t), to which he makes no reference. Factors which influence the colors seen are the nature of the eye, the size of the sectors, the speed of rotation, and the illumination. In direct sunlight he sees no colors at all. He proposes a theory based on the concept that nerves, although conducting the colors seen, cannot conduct more than a certain load. The theory would appear to be improbable

Edridge-Green (30) in 1916 reported that the subjective colors could easily be seen on a black and white design rotating on a kymograph drum. The colors, he says, are the result of after images. It is difficult to understand how, as he says "the after image of black is green."

V. Interest in Non-White Stimuli

In 1917 and 1918 Ives wrote two papers which, together, constitute *the ninth rediscovery* of the subjective colors. Actually, they constitute one of the really great experimental contributions to the field of color, but Ives was not only unaware of their importance but was completely amiss concerning the theoretical implications of his findings. This is all the more interesting, since Ives was a man of monumental proportions in the field of physiological optics.

In the first paper (46), the assumption is made that the visual stimulus is transmitted through a layer of matter having a coefficient of

diffusivity which is different for different wave-lengths, and varies too with the intensity of the stimulus. These theoretical assumptions are discussed in some detail.

In order to investigate further this visual diffusivity of the eye, Ives constructed an apparatus consisting of two concentric circular disks of sheet metal, one outside the other, with a narrow radial slot on each. The relative position of these radial slots could be varied. Colored glasses could be placed in the slots. The speed of rotation of the disks could be varied. A source of illumination was placed behind the disk.

When one slot was of a dark neutral tint while the other was of a lighter neutral tint, and the disk rotated, then the dark slot appeared several degrees behind the other. At very high or very low intensities, this phenomenon was less marked.

When one slot was covered by a blue filter, and the other by a red filter, the blue image lags behind the red image by several degrees. If red, green, and blue filters are used, the red is first, then green, and then blue. Ives writes, "This lagging of one colour behind the other is a very striking phenomenon, well suited to demonstration to a small audience. The effect is most extraordinary if the disk, instead of being rotated, is rapidly oscillated through five or ten degrees, then the blue image may be made to vibrate entirely out of phase with the red, appearing as though attached to it by a cord."

If a purple (mixed) color is revolved, the two components do not resolve (give one slot of red and one slot of blue). Ives explains this by saying, "the difference in diffusivities peculiar to two colors is much reduced when they are simultaneously transmitted to the same retinal area, although it is still present in some degree."

Nevertheless, the following year Ives (**47**) did succeed in resolving a color. He built a new apparatus, which utilized a projection lens system and a rotating mirror. He proposed to determine experimentally whether a mixed yellow resolved into its constituent red and green, and, whether a pure yellow will be resolved into anything at all except pure yellow. Ives found that the compound yellow was *clearly resolved* into red and green by a lateral motion across the field of view. Pure yellow did not separate.

Actually, Ives never did integrate his results with purple and yellow. We may point out that they were obtained with different apparatuses and that may have something to do with the experimental results. But what is important, and what Ives failed to realize, is that he did get a stimulus to resolve. The resolution is probably not caused by the differential diffusivity as Ives supposes, but by the differential rise and fall of the receptors (see **14**). Further work using his technique should yield most valuable results.

In 1922, DuBois (**28**), experimenting with mollusks, noticed that visual receptors respond at different rates to different wave-lengths.

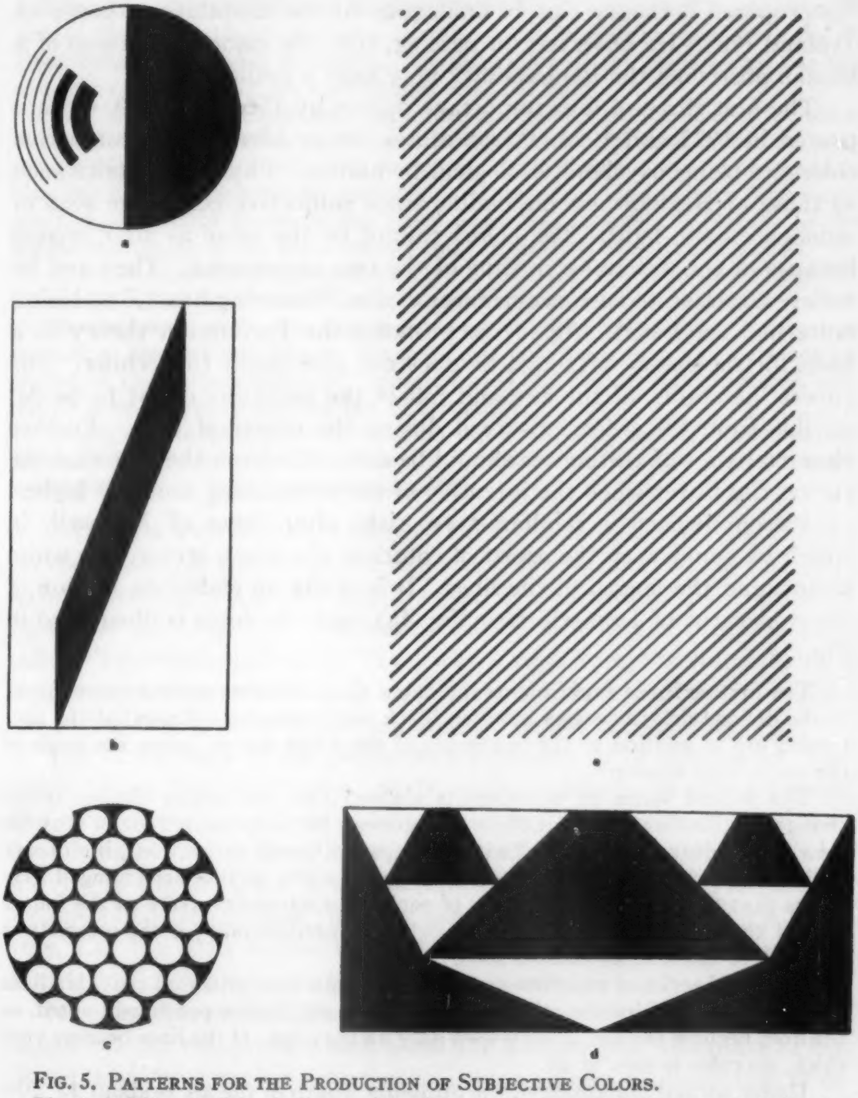

FIG. 5. PATTERNS FOR THE PRODUCTION OF SUBJECTIVE COLORS.

Fig. 5a is Piéron's disk, similar to that of Doniseili. Fig. 5b is Kormann's design for use on a kymograph drum. Fig. 5c is Skinner's disk. The pattern is stationary and is viewed through a pinhole. Fig. 5d is Colenbrander's design for use on a kymograph drum. Fig. 5e is Luckiesh and Moss' design. The pattern is stationary. The colors are seen as pastel shades perpendicular to the parallel lines.

He proposed a theory closely analogous to the modulation theory of Troland (**78**). He observed, in passing, that the color experiences of a human observing the Benham disk may have a similar basis.

The first three of four important papers by Piéron (**58, 59, 60**) appeared in 1922 and 1923. Piéron states the problem of the subjective colors as being psychophysiological in nature. Physical theories such as those of Benham are impossible since subjective colors are seen in monochromatic light. The colors cannot be the same as after images because of the difference in time of the two phenomena. They are, he believes, related to such occurrences as the "fluttering heart," as Helmholtz had suggested. Again, he proposes the Fechnerian theory as a basis for the colors, although he does not give credit to Fechner. The curves, he states, of the rise and fall of the receptors ought to be deducible from the colors observed during the course of time. Further characteristics of the curves should be deducible from the fact that the curves rise faster when the intensity of the stimulating source is higher.

Piéron devised a Benham-type disk, after those of Maxwell, in which he could vary the speed of rotation, the black sector, the white sector, and the length of the lines. It is really an elaborate version of the apparatus of Doniselli (see Fig. 4a), and one form is illustrated in Fig. 5a.

The first series of experiments indicated that the diverse colors correspond to the defined delays (angles between black and beginnings of arcs) of the generating arc in relation to the beginning of the white sector, when the angle of the arc is kept constant.

The second series of experiments showed that the colors change under changes in illumination. But these changes can be compensated for so that the color will appear constant (but with perhaps a different saturation), if either or both the speed of rotation and/or the angular white section are changed. He shows that the characteristic delay of each color varies inversely as the fourth root of the intensity of the stimulus light. Experimentally, if the intensity is too high or too low, no color is seen.

The third series of experiments on the Benham disk indicated that thin lines show optimum subjective colors. As Finnegan and Moore previously noted, as the lines become thicker, color is seen only on the edge. If the lines become very thick, no color is seen at all.

Under normal illumination, the optimum length of the arc is about 45° (the lengths of the arcs of the Benham top). If the arc is 180°, no color is seen at all. Less color is seen at angular lengths between 180° and 45°, and less than 45°.

The fourth series of experiments showed that the amount of white sector following the lines has little influence on the colors seen. When the white precedes the lines, the amount of white determines the colors, with the exception of blue, which is always found in the terminal position. Several disks are produced in Piéron's paper which, although widely different, illustrate such equivalence of color. The shortening of the black sector when the speed of rotation is

sufficiently rapid, weakens the blueing effect of the "privileged" terminal position of the lines. Under the same conditions the leading arc goes from red to green because the delay is longer. The elongation of the black sector has the opposite effect on both the leading and terminal lines.

The fifth series of experiments involved interpolating a segment of the black sector in the white sector, and on a different disk, the reverse procedure. Insertion of a small black sector in the middle of the larger white sector tends to blue the lines which precede, and redden those which come after. The interposition of a white phase on the black sector has an effect analogous to the simple shortening of the black sector, and the result is as described above.

In the sixth series of experiments Piéron compared the color appearances of the Benham disk under radiations through Wratten filters (running the gamut of the spectrum), with color appearances under white light, in much the same manner as Stewart and Abney. He finds that colors are seen which are not included in the "monochromatic" radiations, as did the previous investigators, but he is unable to derive any law of formation.

Finally, if a negative of the Benham top (similar to that of Finnegan and Moore, see Fig. 1o) is rotated, colors are never seen on the white arcs, although colors are seen on other parts of the disk.

From his experimental results, Piéron attempted to deduce the curves of the rise and fall of the receptors. The curves, which change in form depending upon the illumination, are essentially like Stewart's third case (see Fig. 3c), with the difference that Piéron proposes a so-called "hypermaximal stage," *i.e.*, where each receptor responds to a high point before reaching an equilibrium. The receptors invariably rise in the order red, green, blue, and fall in the reverse order.

The black rings of the Benham disk appear to be colored by "diffusion" from the neighboring white surfaces which are being pulsed. The color seen on the lines is determined by the amount of activity in the lines and the resulting amount of irradiation. Thus, if there is no activity in the lines, *i.e.*, the lines follow the black directly, red is seen because of the strength of the diffusion. If there is some activity in the lines, *i.e.*, the lines follow white which follows the black, then some other color is seen because there is less diffusion.

In monochromatic light the color pulsed tends to stimulate and fatigue certain receptors, which allow the antagonistic receptors to discharge, and the complementary color is seen. Thus it follows that stimulation at first tends to influence the receptors most highly tuned to the stimulus, and thereafter the complementary receptors. Piéron's experimental results are quoted to prove this hypothesis.

Piéron concludes by formulating two laws which will predict all colors of the Benham disk:

Law I. Under the influence of a luminous excitation of the retina by a complex radiation which integrates to make white there is first produced a chro-

matic disequilibrium with successive predominant shades running from red to blue in order of the spectral colors, because of an unequal speed of establishment of the fundamental chromatic processes released by this excitation until the hypermaximal transitory state is attained.

Law II. When a small retinal surface is not, or is only feebly excited, in the neighborhood of a region which is the site of a luminous and chromatic excitation, the excitation of this surface by diffusion shows a predominance of chromatic processes which are propagated with an intensity much greater than the luminous processes.

Forester (35) pretended to use the Benham disk in proof of his highly improbable theory of color vision. He proposes two levels of excitation in the nervous system relating to color perception. The first is in the retina, called the R level, and the second is in the optic tract, and is called the T level. White light has the interesting property of being able to by-pass the R level and influence only the T level. It nullifies the centers at the T level, and prohibits them from being influenced by the receptors at the R level. White is the physical stimulus on the rotating Benham disk (with the exception of the black sector) except in one place—where the lines are on the white surface. The red receptor can and does stimulate this spot, since the receptors are not nullified at the T level but why it should be red and not some other color seen is not made clear. The theory makes extensive use of the concept of Lapicque's chronaxie in its exposition.

Pauli and Wenzl (56) in 1924 also attempted an experimental analysis of the colors produced on the Benham disk. The experimental variables were the composition of the disk (distribution of black, white, and colored sectors), number of arcs, size of the segments and openings, diameter of the disk, color of the arcs, color of the dark sector, and speed and direction of rotation. These factors were varied by means of a Maxwell-type disk.

Pauli and Wenzl report that the colored appearances of the Benham disk can be divided into three groups based on the speed of rotation: the first at 3–5 rotations per second, the second at 5–37 rotations per second, and the third at 33–40 rotations per second. The colored appearances are given in great detail and will not be discussed here—except to mention that in the second phase a mosaic pattern is seen, which may be the hexagonal pattern seen by others.

If the white field is replaced by a colored field, after the manner of Bagley, the black sector shows complementary colors. If the black field is replaced by a colored sector, the colors are still seen, but they may be altered somewhat, depending on the particular color field substituted.

Subjective colors may be seen during tachistoscopic exposure.

Under these conditions the colors show a decrease in intensity and distinctness, and can be seen only at a fast speed of rotation. In direct sunlight many of the colors disappear completely. When the disk is viewed peripherally, no colors may be seen. In general, Pauli and Wenzl's other results agree with former investigators.

Pauli and Wenzl, in their theoretical explanation, state that after images are not responsible for the colors. They theorize that each of the receptors has a typical inertia, and consequently a typical natural period. When the frequency of the pulse is that of the natural period of the receptors, it will respond. Pauli and Wenzel, after the manner of Piéron, also postulate an induction phenomenon in explaining the spread of the colors. "We have," they say, "an overlaying of three factors; primary excitation, resonance and number of excitations, and the induction effect."

Von Kries (81), writing in the 1924 American translation of Helmholtz, pointed out that Helmholtz' earlier description of the subjective colors could be applied to the Benham top.

Stewart (77) in 1924 wrote his second paper on the subjective colors. Observations were made by looking at a rotating mirror which reflected the light source. The colors appeared as luminous bands, and, in the main, the color appeared homogeneous over the entire surface except at the edges. However, the color phenomena on the edges of the band were essentially the same as the rest of the band, but different in phase, because the edges were illuminated by slightly less intensity.

In general, a white light of given intensity may be pulsed at progressively increased speeds so that the subjective color sensation passes progressively from violet to red approximately in the ordering of the spectrum. The colors after the green on the red end of the spectrum are less pure than the others because the pulse rate is rapidly approaching the fusion point. However, under suitable conditions, when the illumination is carefully controlled, a very good red can be obtained. It can further be shown that when the pulse rate is held constant, the same succession of colors can be obtained by adjusting the intensity of the light; when the physical intensity is increased the colors shift toward the violet and when the physical intensity is decreased, the colors shift toward the red.

Color effects can be observed by looking at a black and white picture through a black, white, and sectored disk (similar to Bidwell, Fig. 1u). The color effects are most determined by the physical intensity of the illumination of any given portion of the picture, in other words, the shading. Stewart writes:

It is easy to cause the darker foliage in an engraving of a landscape to assume a green color while the less shaded water or sky is violet or bluish, with the details of the shading represented by corresponding nuances of the subjective color. Then by a change of speed of the disk a complete change can be produced in the colors, which again, however, are appropriately placed on the different parts of the picture according to the depth of the shading. The colors are so easy to obtain that I have seen children amuse themselves by the hour "coloring" pictures in books or newspapers subjectively by observing them through a rotating disk, and describing the colors correctly. While it is generally not difficult to clothe the landscapes in colors which are appropriate, or at least, not glaringly incongruous, it is of course different with pictures in which human faces or figures are the important element. With the shading below the chin perhaps a light green (at a particular speed of rotation), the cheeks violet or bluish, the hollows of the eyes perhaps green, the hair a dark green, and various garments colored according to their shading (creases, for instance, at a given phase green), and then everything changing phase with an increase or dimunition in the speed, the personal appearance of a state beauty, the statesman or criminal of the hour, the horse which has just won the Derby, or the reigning pugilist, is apt to be somewhat bizarre, and therefore rather amusing for children. On the other hand black and white pictures of such objects as embroidery, tapestry, Scotch tartans, carved work, stained and cut glass, etc., may show curious and even beautiful appearances, the subjective colors imparting a certain richness to the designs. Of course, unless the black and white pictures were expressly produced in order that the subjective tints might approximate the actual colors at some rate of intermission (a feat perhaps in simple cases theoretically possible), the subjective colors could only by accident be correct.

Stewart includes a rather elaborate discussion of his observations of a rotating Benham top. In general his results agree with those of previous investigators. When the lines are observed through a magnifying glass, the color is the same as when observed with the naked eye. When the Benham top is held stationary and observed through a rotating sectored disk, the color appearances on the Benham top are about the same as those seen when the top itself is rotating. Of course, all the arcs appear to be the same color, but the color can be made to change depending on the speed of rotation of the sectored disk. In experiments utilizing Benham disks with arcs of varying thickness, colors can be seen throughout the arc no matter what the thickness, although different colors are seen in different portions—a function of the speed of rotation. Color blind individuals do not see the subjective colors with the same clarity as normal individuals.

Stewart conducted experiments wherein colored lights were observed in the rotating mirror and through the rotating disk. He also viewed colored pictures under the same conditions. He reports that with a slow pulse rate, the blue tints are strengthened, white becomes violet and the greens and reds are weakened. As the pulse rate is in-

creased, the blue remains predominant and the greens are strengthened. At a high rotational speed, the reds prevail. The color changes in the picture are primarily dependent on the physical saturation, hue, and brightness of the picture; the Purkinje phenomenon is not a factor.

Concerning the effect of the subjective color sensations on evolution, he writes:

> The question suggests itself whether the color sensations of animals in an environment habitually or frequently illuminated by flickering light, as in forests or not far below water surfaces, could be modified at all by such phenomena as those under discussion. If so, protective color adaptation might in some degree be expected under these circumstances to take account of the factor of subjective coloration. Protective coloration has considerable interest in connection with the question of color vision in animals. For it seems in some cases to afford a means of testing whether the response in sensation to given wave-lengths is approximately the same as in man. Whether the subjective modifications of color sensation caused by intermission at appropriate rates enter at all as a factor into such phenomena as the aurora borealis, or some other of the solar color phenomena, depends upon the magnitude of any intermittent changes of luminosity concerned in them and their period. Where the spectral characters of the light sufficiently explain the perceived colors there is of course no room left for the subjective factors.

Piéron (**61**) published his fourth paper on the subjective colors in 1928. Essentially, it was a modification of the work of Abney. He observed in a most systematic fashion a rotating Benham top under various white lights. These white lights were always composed of two near monochromatic radiations. The whites looked alike; their spectral compositions were vastly different. Finally, the top was observed under a yellow source, composed of red and green radiations.

Piéron's results conclusively show that the color seen is a function of the spectral composition of the illuminating source and not of its tristimulus values. The results give rise to two principles. (1) The dominant radiations of the spectral composition of the illuminating source tend to become accentuated. (2) The complement of the dominant radiations tends to be evoked. The second principle may be in opposition to the first in that it may create a color which corresponds to absent radiations.

Kormann (**50**) in 1931 described a "new" type disk which revolved on a kymograph drum. He was apparently unaware of the similar work performed by Doniselli and Piéron. He presents three new designs, one of which is illustrated in Fig. 5b. He points out that if the design were transferred to a disk, the result would be similar to a half moon—like Wolf's disk (see Fig. 1t) which he does not mention. Kormann compared his artificial spectrum with a prismatic spectrum and reported

that the colors occur in the same spectral order, with the exception of the blue and violet, which are inverted. No colors are seen at high intensities.

VI. Rediscoveries in 1931–32

Klopsteg (49) in 1931 made *the tenth rediscovery* of the subjective colors. Observing a pulsing neon tube behind a ground glass, he observed a great variety of colors. When an electric fan was placed in front of the screen, the edges of the blade appeared colored when the appropriate stroboscopic effect was produced

Way (82) followed Klopsteg with *the eleventh rediscovery*. He pointed out that he independently noted the phenomenon while running a 16 mm. motion picture projector without film at a speed of about eleven frames per second. He observed the color effect on the screen.

Lemmon (51) in 1932 indicated that the colors observed by Klopsteg and Way were probably the subjective colors, known for over one hundred years.

Skinner (71) made *the twelfth rediscovery* in 1932, observing the disk shown in Fig. 5c. The disk is stationary and is to be viewed through a pinhole. Skinner writes, "So far as I am aware, no comparable effect has been reported." Skinner's explanation, that the colors are due to areas with so few elements being affected that a disproportionate number of cones of a single type are stimulated, is not probable in the light of measurements of the cone size by Østerberg.

VII. The Modern Experimental Work

Colenbrander (24) in 1933 wrote a very lengthy paper on the subject, but the effort was certainly greater than the result. He observed a Benham disk, as so many before him, and developed several new designs to be placed on the kymograph drum. One of the new patterns is illustrated in Fig. 5d.

With respect to the Benham disk, he reports the following observations. A convex lens distorts the colored appearance; the colors are therefore not physical in nature. The colors can be seen under a monochromatic light, a fact reported by Benham, but to whom Colenbrander makes no reference. The theory of Henry, that the colors are caused by the distribution of the retinal elements into zones can be dismissed immediately by covering individual parts of the disk.

It is disappointing that the great number of kymograph designs and their accompanying experiments produced so few conclusions. In general, the conclusions agree with what was known before—that the colors

are never very saturated and are usually transitory in nature, with brighter illumination the colors are pushed toward the red side, etc.

In his theoretical explanation, Colenbrander follows the familiar Fechnerian theory of the rise and fall of the receptors. Further, all the color appearances can be partially explained as deviations from the Plateau-Tablot law, that complementary colors (in this case black and white) mix to give gray. Thus at lower speeds, the law does not hold and the darkening and lightening of the various colors is brought about.

Luckiesh and Moss (**54**), in 1933, published a series of parallel lines, illustrated in Fig. 5e, for use as a test of vision. They noted color appearances over the surface of the figure, suggested that they were due to eye movements, and that they were similar to the colors of the Benham disk.

Fry (**36**) published the first of four papers on this subject in 1933. The entire series attempts to show that the observed phenomena of subjective color vision are not incompatible with Troland's (**78**) theory of the optic nerve current. Troland suggested that impulses passing along a given fiber could be "... spaced in different characteristic ways to represent the various colors, luminosity still depending upon the total number of pulses per unit time." These modulations have four characteristics: (1) the average frequency of impulses; (2) the frequency; (3) the form; and (4) the amplitude of the modulation waves. Possibly intensity depends upon (1), hue on (2) or (3) or both, and saturation on (4). White is no modulation; black is no activity at all. Fry suggests that experiments by Adrian and Matthews (**3**) on the conger eel, similar to those of DuBois (**28**) which he does not mention, showing that the optic nerve modulates when stimulated by intermittent light, may be a basis for the subjective colors.

The rotating disk used by Fry is illustrated in Fig. 6a. The dotted line indicates the area behind the disk which is illuminated. The angular cut-out sector at B, the speed of rotation, and the level of the rear illumination can all be changed. The surface illumination of the disk is held constant.

Employing rotational speeds ranging from 4–16 per second, and sector openings ranging in time from .46 sigma to 6.94 sigma, he observed colors which were blue, purple, violet, pink, and white. Keeping the speed of rotation constant, but varying the sector opening and intensity of *B*, he noted colors, which fulfill the following law:

For a given duration of stimulation increasing the intensity causes the color to change progressively from green toward pink. Below a certain level, however, decreasing the brightness of *B* causes the color to change to gray. For a

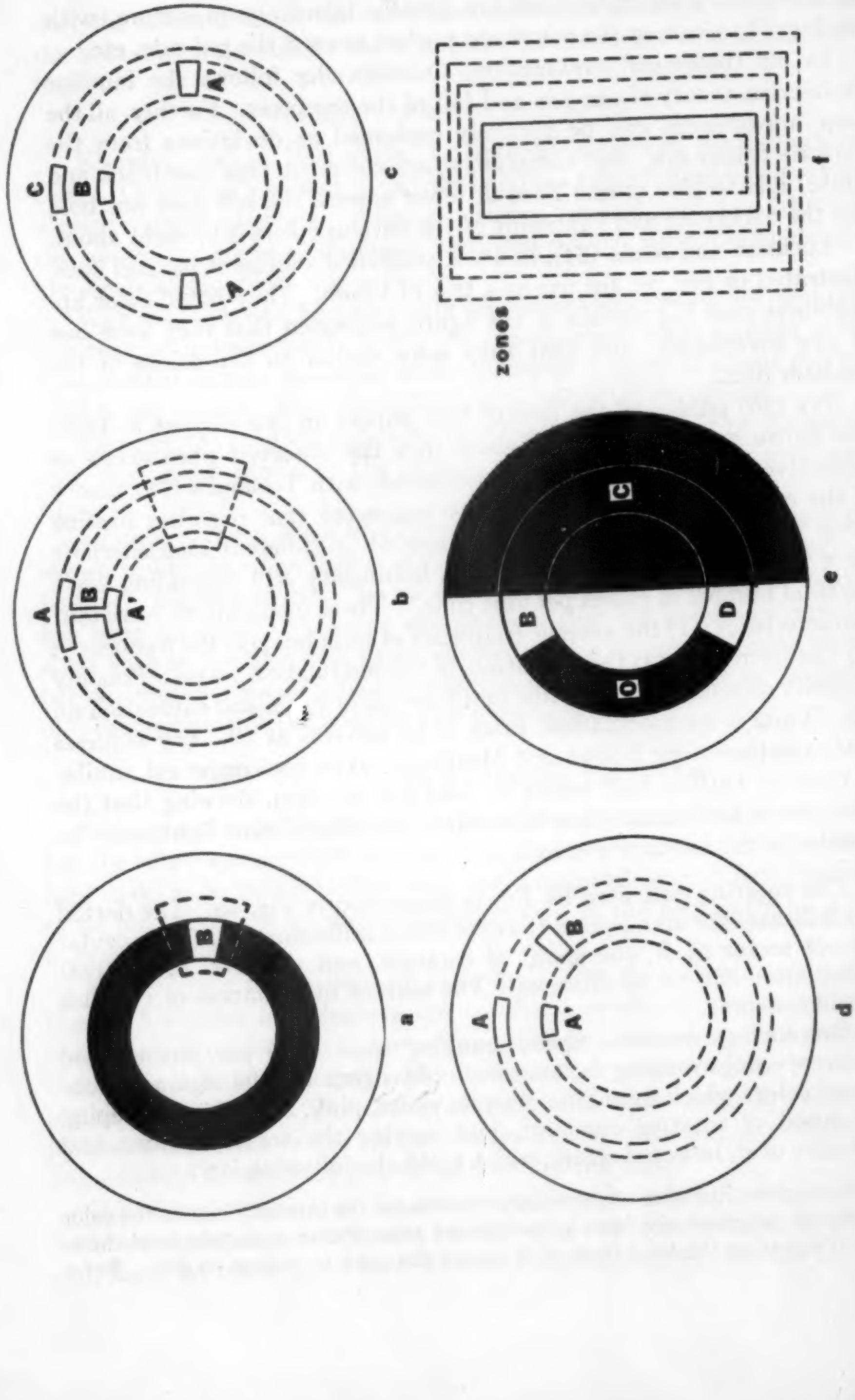

FIG. 6. THE SPECIALIZED CHARACTERISTICS OF THE SUBJECTIVE COLORS. Figs. 6a–6e are Fry's disks illustrating the effects of interaction. Figs. 6a–6d are sectored. Fig. 6f depicts Gebhard's zoning effects.

> given intensity of stimulation increasing the duration causes the color to change progressively in the direction from green toward pink. . . Beyond a certain limit, however, increasing the duration causes the color to change to white.

For the final experiment in this paper, the surface illumination of the disk was made both brighter and equal to *B*. Pink can be produced without the aid of bright surroundings, while purple, blue, and green cannot.

All of these experimental results, some of which were known before, are exceptionally interesting and provocative. The fact that they do not disprove the modulatory theory is only of minor importance since they do not disprove a number of other theories, for example, that of Forster (**35**). Actually, in science, we ***never*** prove that a theory is right, but only that it is not wrong. Fry's analysis in terms of the modulation theory would have been a more powerful document had it shown the Fechnerian theory to be wrong.

Fry's (**37**) second paper in 1933 makes inquiry concerning the stimulated retinal area, beta, and its surround. If beta and the surround can be pulsed or not pulsed, there are four possible combinations: beta is steady, surround steady; beta steady, surround intermittent; beta intermittent, surround steady; beta intermittent, surround intermittent. If it can be shown that the color seen is dependent solely on the intermittence of beta, then no other facts need be invoked for an explanation. The color seen in beta can be considered to be a color contrast phenomenon if the color is independent of the pulsing of beta and dependent on the pulsing of the surround. The level of the intensity of the surround, whether it pulses or not, can be shown to affect the colors seen in beta.

Fig. 6b illustrates Fry's disk for this series of experiments. *A*, *B*, and *A′* indicate slits. The dotted lines indicate an illuminated background. When *A*, *B*, and *A′* are all intermittent, *A* and *A′* appear pink and *B* appears green. When *A* and *A′* are steady and *B* is intermittent, then *A* and *A′* appear pink and *B* appears green. When *A* and *A′* are pulsed, and *B* is intermittent, then *A* and *A′* appear pink, and *B* still appears green. The contrast effect is non-operative. Fig. 6c indicates an additional stimulus pattern. *A*, *A′*, *B* and *C* are slits. When the disk is rotated, *b* is seen as yellow. If *b* remains steady, and *A* and *A′* are intermittent, *b* still appears yellow. If *A* and *A′* are steady, then *b* appears blue. The yellow of *b* is a contrast phenomenon. Similarly, the contrast effect by *b* is responsible for the colors seen in *A* and *A′*. Fig. 6d indicates another stimulus pattern illustrating interaction. When rotated in a counterclockwise direction, *A* and *A′* appear purplish blue, and *B* appears yellowish white. In a reverse direction, the colors are reversed.

Fry concludes this paper with a discussion of the Benham top, adding that the red seen is due, at least in part, to chromatic aberration.

On a different disk, subjective red borders, when viewed through a convex lens, appear to be blue.

Fry takes issue with Piéron's concept that the leading white sector is the determiner of the color. He observes that a disk shown in Fig. 6e, when rotated, will shown green. It will still show green no matter how much the inner circle *CBDO* is rotated with respect to the rest of the disk. It is only when the absolute amounts of white at *B* and *D* are changed that the color is changed.

Fry's (**38**) third paper, published in 1935, attempted to show that the trichromatic theory (our Fechnerian theory) was inadequate in its explanation of the subjective colors. He constructed a rather elaborate apparatus which presented the same spectral source to two fields, which could be observed by the subject. The intensity of one field could be varied by adjustment of the sector slit of an episcotister, while the intensity of the other could be varied by adjustment of the speed of rotation of another episcotister. The conditions of equivalence between the two fields were studied for different brightnesses of ground and for different wave-lengths. That the ground is important is illustrated by a series of curves, but their theoretical implications need not concern us here.

A more important series of curves is that which designates equivalent brightnesses between the two fields when wavelength is varied. According to Fry, the fact that the red response is relatively weak finds basis in how one interprets the response of the receptors. If the red response is due primarily to the response of the red receptor, then we can say that the red receptor is weak. Experimentally, he demonstrates that a bluish green sensation can be obtained from white light which contains all the colors of the spectrum, and also from white which is composed only of blue and green. This may give support to the concept of the feeble response of the red. Three facts remain unexplained. Colors other than blue green can be obtained with an intermittent white light. If the stimulating light is of sufficient intensity, or the exposures are long enough, or both, then a pink sensation results. The blue green sensation cannot be produced unless the surround is of high intensity. Fry concludes, "As in the case of the eye, so also in the case of the photographic plate (15), intermittent exposure is less effective than continuous exposure for all wavelengths, and the longer waves suffer more than the rest in this respect." The three component theory, says Fry, offers no ready explanation for these facts.

Fry's (**39**) fourth paper, published in 1936, makes use of an apparatus similar to that described in his third paper. Fry investigated the subjective colors seen under pulsing monochromatic lights, and under pulsing monochromatic lights whose saturations were reduced by a non-pulsing white light. The effects of varying the wavelength and frequency of the pulse were investigated. The experiments are similar to those of Abney and Piéron, to which Fry makes no mention. In

general, the color seen is that of the hue of the wave-length of the stimulating light. There is, however, a tinge of blue and violet, in the secondary images, thought to be similar in nature to Bidwell's ghost. When stimuli of lower saturation (as described above) are pulsed, the secondary images tend to be complementary to the primary stimulus.

Erb and Dallenbach (31) presented a most important contribution in 1939. They proposed to study the Luckiesh-Moss stimulus pattern (see Fig. 5e) to determine the effect of knowledge and instruction, color of the pattern and color of the background, the width of lines and the spacings, and eye movements on the subjective colors.

Observers with and without knowledge of the subjective colors were shown the stimulus pattern with three different types of instructions. One group was told to simply observe the pattern, the second group was told to use far fixation, and the third group was told to look for colors. Knowledge of the phenomenon favors the subjective colors, but it by no means guarantees that they will be seen. All observers told to look for the colors saw them. Phenomenal movement was reported by very many subjects: streaming, random movement, moving band and zig-zag lines, movement of stimulus lines, circular movement, and shimmer. There is no relation between the subjective colors and the subjective movement.

The pattern was printed in red, yellow, green, blue, black, and white upon red, yellow, green, blue, black, neutral gray, and white backgrounds. It was also printed upon various stocks of paper. The following combinations were found to give the optimum colors: black lines on white background, blue on white, green on white, black on green, black on yellow, and green on yellow. The best of these is black on white.

Narrow lines and narrow spaces give saturated hues; narrow lines and wide spaces, or the reverse, give less saturated colors. Broad lines and wide spaces give no color at all.

When the stimulus pattern is placed in the Dodge tachistoscope, and the exposure is very short, no colors are seen. An exposure of very much greater length than that required for eye movements is necessary before the subjective colors are observed. It follows, that although eye movements are a necessary factor, they are not sufficient or compulsory.

Abel (1), a student of Dallenbach, continued these researches in 1939. Her problem was to observe the subjective colors of the same stimulus pattern under monochromatic and polychromatic illumination. The following light sources were used: a tungsten lamp radiating a continuous spectrum; a mercury arc lamp with its lines at 577, 579, 546, 492, 436, and 405 millimicrons; an amber filter which transmitted almost exclusively the 577 and 579 millimicron lines from the mercury arc; a green filter which transmitted the 546 line of the mercury arc; and a red filter, used with the continuous source, with a sharp cut off at 623 millimicrons. Abel writes:

The main results may be classified in the following manner: (1) Differentiation among the black diagonal lines both of hue and of brightness. (2) Colors in the interspaces between the black lines either filling in all the space between the lines or merely along the edges of the lines. (3) Pattern lines diagonally across the parallel black lines. Sometimes these patterns were black or gray but more often colored pastel shades. (4) When colored phenomena were reported, continual change of the color was observed generally in the direction of an increase in saturation.

Under all conditions, both with polychromatic and monochromatic lights, with the exception of monochromatic red, many subjective colors were reported. In polychromatic light, the colors were usually more saturated than they appeared in monochromatic light. The colors seen were not necessarily restricted to the after image of the physical color. With red light, only various shades of red are seen. These results are in direct opposition to those reported by Fry.

Segal (67), a student of Piéron, in 1940, independently hypothesized the modulation theory of Troland, but did not mention either Troland or Fry. He too points out that the subjective color appearances are not incongruous with the theory.

Gebhard (40) contributed the most recent, thorough communication in 1943. He utilized the left side of an apparatus described by Brown (17). The subject observes a figure and ground. For both this figure and ground, it was possible to vary the luminosities independently, the frequencies of intermittence, the on/off ratios, and the phase relationships. Thus was allowed a great variety of experimental conditions. Three kinds of illumination were used: a white continuous source, a Wratten red filter which transmitted nothing below 650 millimicrons, and a filter combination for wave-lengths at 560 millimicrons.

Observations were made with white, red, and green lights, employing various frequencies and conditions whereby the ground had a phase lead over the field, and vice versa. For two of the three observers, the red and green stimuli produced no subjective sensations other than those attributable to the physical characteristics of the stimulus. The third observer saw a multitude of colors. It is possible, Gebhard suggests, that the difference was due to the observer's greater astigmatism. This led to the second problem.

The boundary between the field and ground was made indistinct. Under these conditions all observers saw colors—some where they had not seen them before. The field was now made into a rectangular bar whose width could be varied. Now, "the area of the figure and the part of the field immediately adjacent to it, was found to divide itself symmetrically into a series of zones or bands parallel to the two lateral boundaries of the bars." This effect is illustrated in Fig. 6f. The wider the bars, the greater the number of zones which are produced. When the illumination was reduced, two of the three subjects could not see zones. Practice and fatigue are necessary for the optimum perception of the zones. Simultaneous with the zoning effect, there were many

field color effects. Geometrical patterns are mentioned again, and these may be the same as Fechner's hexagonal cells.

Gebhard attempts to show the inadequacy of the Fechnerian theory by saying, "Colors are producible under various conditions of stimulation with monochromatic red illumination so chosen as to act upon the R retinal process only." One wonders why Gebhard assumes that he is stimulating one retinal process with a filter transmitting 650 millimicrons and above. The further inadequacy is shown by, "The slow arousal of the zoning effect with blurring and the general instability reflected in long term fluctuation of the zones." Gebhard continues, ". . . it is doubtful if the modulation hypothesis is sufficiently comprehensive to deal with the formation of multiple boundaries, the intermittent and steady field effects, the instability of the colors and zones and the long term developmental effects."

Finally, Brown and Gebhard (**18**) in 1948 reported several new color phenomena, some which were of a binocular nature, which may be related to the subjective colors.

The present paper has made no attempt to cover the literature on the direct measurement of the rise and fall of the color receptors. An excellent survey of this material may be found in Bills (**14**).

VIII. Summary

1. It is suggested that the Prevost-Fechner-Benham subjective colors may be used as an outside criterion for the determination of the axes in the color space.

2. The subjective colors were discovered by Prevost (**62**) in 1826. They were rediscovered by Fechner (**33**), John Smith (**73, 74**), Brewster (**16**), F. J. Smith (**72**), Hannay (**42**), Stewart (**76**), Charpentier (**21**), Benham (**84**), Ives (**46, 47**), Klopsteg (**49**), Way (**82**), and Skinner (**71**)

3. Subjective colors may be seen in any white light which can be made to pulse below the fusion frequency. Such colors have been reported by Prevost (**62**), J. Smith (**73, 74**), F. J. Smith (**72**), Hannay (**42**), Stewart (**76, 77**), Charpentier (**21, 22, 23**), Bidwell (**11**), Percival (**57**), and Fry (**39, 40**).

The colors are most easily seen on rotating black and white disks of various designs. Such disks (or designs on kymograph drums) have been presented by Fechner (**33**), Dove (**26, 27**), John Smith (**73, 74**), Helmholtz (**43**), Brücke (**19**), Benham (**84**), Finnegan and Moore (**34**), Hurst (**45**), Wolf (**85**), Baumann (**6**), Doniselli (**25**), Edridge-Green (**30**), Kormann (**50**), and Colenbrander (**24**).

Colors have been obtained from sectored disks by Rood (**63**), Brewster (**16**), *La Nature* (**41**), Bidwell (**11**), and Fry (**36, 37**).

Colors from black and white pictures, when viewed under a pulsing light, have been reported by Stewart (**77**).

Colors from stationary patterns, due to eye movements, have been reported by Skinner (**71**) and Luckiesh and Moss (**54**).

4. Almost all investigators report that the type and level of illumination, the speed of rotation or frequency of pulses, the design or distribution of black, white, and lines, and the state of the eye affect the colors seen.

Movement of some kind appears to be a necessary factor. That brief tachistoscopic exposure shows no colors at all is reported by Pauli and Wenzl (**56**) and Erb and Dallenbach (**31**).

That practice aids in the perception of the colors is reported by Helmholtz (**43**), Aubert (**4**), Gebhard (**40**), but Bagley (**5**) insists that practice is not important. Erb and Dallenbach (**31**) show that knowledge of the colors facilitates their observation.

That there are differences in the colors seen in direct and indirect vision is reported by Aubert (**4**), Exner (**32**), and Pauli and Wenzl (**56**).

Stewart (**77**) reports that color blind individuals do not see the colors with the same clarity as normal individuals.

Induction phenomena (subjective colors induced by the stimulation of an adjacent retinal area) are reported by Bagley (**5**), Piéron (**58, 59, 60, 61**), Pauli and Wenzl (**56**), Fry (**37**), and Gebhard (**40**).

5. A hexagonal cell structure, seen with the subjective colors, has been reported by Fechner (**33**), Brewster (**16**), Helmholtz (**43**), Aubert (**4**), Exner (**32**), Edridge-Green (**29**), and Gebhard (**40**).

6. Subjective colors from non-white stimuli fall into three classes. Those who reported subjective colors from general chromatic stimuli are Dove (**26, 27**), Rood (**63**), Helmholtz (**43**), Hannay (**42**), Stewart (**76, 77**), Benham (**9**), Liveing (**53**), Ives (**46, 47**), Piéron (**58, 59, 60, 61**), Pauli and Wenzl (**56**), Klopsteg (**49**), Colenbrander (**24**), Erb and Dallenbach (**31**), and Gebhard (**40**). Those who reported subjective colors from general chromatic stimuli with the exception of red are Abney (**2**) and Abel (**1**). Fry (**39**) reported that he was able to get no subjective colors from chromatic stimuli, except those of after images.

7. The theory of the differential rise and fall of the receptors was proposed by Fechner (**33**), and supported by Helmholtz (**43**), Hannay (**42**), Stewart (**76, 77**), Liveing (**52**), Sexton (**68**), Percival (**57**), Piéron (**58, 59, 60, 61**), and Colenbrander (**24**).

The modulation theory of the optic nerve is supported by DuBois (**28**), Troland (**78**), Adrian and Matthews (**3**), Fry (**36, 37, 38, 39**), and Segal (**67**).

Other theories of a physiological nature have been suggested by Prevost (**62**), N. Smith (**75**), Charpentier (**22**), Gray (**86**), *La Nature* (**41**), Henry (**44**), Bidwell (**11**), Bagley (**5**), Doniselli (**25**), Baumann (**6, 7, 8**), Edridge-Green (**30**), Ives (**46, 47**), Piéron (**58, 59, 60, 61**), Forster (**35**), Pauli and Wenzl (**56**), and Colenbrander (**24**).

Other theories of a physical nature have been proposed by J. Smith (**73**, **74**), F. J. Smith (**72**), Benham (**9**), and Schwartze (**66**).

Discussion

This paper has outlined the long history of subjective colors. It may be that one day these same colors may solve the great enigma of color vision—the specification of the receptors of the eye.

Throughout the history of physiological optics the eye has never resolved a stimulus into its components under any other experimental conditions. The receptors could never be determined directly because the eye consistently integrates the many wave-lengths of the stimulus to perceive it as an unanalyzed sensation. This integration must require the most elaborate mechanism—since it fulfills this ideal even under the most elaborate experimental conditions, with but a single exception.

But it is just possible that the eye can be made to yield its secret. If the subjective colors are due to the differential rise and fall of the receptors, then here at last the eye is not so perfect after all, for it is not completely integrating the stimulating wave-lengths. If the Fechnerian theory of the subjective colors is true, then we need but explore the color space until we find colors which when pulsed, show no subjective colors. When such colors are found, then we are *stimulating one receptor*. If it be that the receptors are imaginary, as is probably the case, then data may be taken on the real colors, and extrapolated into the color space which corresponds to no physical stimulus. Of course, the experimental variables affecting the subjective colors, which have been discussed at length in this paper, would have to be carefully controlled.

BIBLIOGRAPHY

1. Abel, T. M. 'Subjective' colors from line-patterns viewed in polychromatic and monochromatic light. *Amer. J. Psychol.*, 1939, **52**, 610–615.
2. Abney, W. de W. The artificial spectrum top. *Nature, Lond.*, 1894, **51**, 292.
3. Adrian, E. D., & Matthews, R. The action of light on the eye. *J. Physiol.*, 1927, **63**, 378–414; 1927, **64**, 279–301; 1928, **65**, 273–298.
4. Aubert, H. *Physiologie der Netzhaut.* Breslau: E. Morgenstern für Aug. Schultz und Co., 1865. P. 355 and pp. 377–380.
5. Bagley, F. W. An investigation of Fechner's colors. *Amer. J. Psychol.*, 1902, **13**, 488–525.
6. Baumann, C. Beiträge zur Physiologie des Sehens: IV. Subjektive Farbenerscheinungen. *Pflüg. Arch. ges. Physiol.*, 1912, **146**, 543–552.
7. Baumann, C. Beiträge zur Physiologie des Sehens: V. Subjektive Farbenerscheinungen. *Pflüg. Arch. ges. Physiol.*, 1916, **166**, 212–216.
8. Baumann, C. Beiträge zur Physiologie des Sehens: VII. Subjektive Farbenerscheinungen. Subjektives und objektives Empfinden. *Pflüg.*

Arch. ges. Physiol., 1918, **171**, 496–499.

9. BENHAM, C. E. The artificial spectrum top. *Nature, Lond.*, 1894, **51**, 200.
10. BENHAM, C. E. The artificial spectrum top. *Nature, Lond.*, 1895, **51**, 321.
11. BIDWELL, S. On subjective colour phenomena attending sudden changes in illumination. *Proc. roy. Soc.*, 1896, **60**, 368–377.
12. BIDWELL, S. On negative after-images following brief retinal extinction. *Proc. roy. Soc.*, 1897, **61**, 268–271.
13. BIDWELL, S. Some curiosities of vision. *Notices of the Proceedings at the Meetings of the Royal Institution of Great Britain*, 1896–98, **15**, 354.
14. BILLS, M. A. The lag of visual sensation in its relation to wave-lengths and intensity of light. *Psychol. Monogr.*, 1920, **18**, No. 5.
15. BLAIR, J. M., & HYLAN, M. C. The intermittency effect in photographic exposure. *J. opt. Soc. Amer.*, 1933, **23**, 353–358.
16. BREWSTER, D. On certain affection of the retina. *London, Edinburgh, and Dublin Philosophical Magazine and Journal of Science*, 1861, **21** (4th series), 20–24.
17. BROWN, C. R. A 'universal' apparatus for research in physiological optics. *J. exp. Psychol.*, 1943, **33**, 340–349.
18. BROWN, C. R. & GEBHARD, J. W. Visual field articulation in the absence of spatial stimulus gradients. *J. exp. Psychol.*, 1948, **38**, 188–200.
19. BRÜCKE, E. Über die Nutzeffect intermitterender Netzhautreizungen. *Sitzungsberichte der Mathematisch-Naturwissenschaftlichen Classe der Kaiserlichen Akademie der Wissenschaften*, 1864, **49** (Abtheilung 2), 128–153.
20. C(ATTELL), J. McK. The spectrum top. *Science*, 1895, **2** (new series), 13.
21. CHARPENTIER, A. Phénomènes de coloration apparente observés sous l'influence d'excitations lumineuses instantanées. *C. R. Soc. Biol. Paris*, 1891, **3** (9 sèrie), 596–599.
22. CHARPENTIER, A. Remarques et expériences au sujet de la coloration entopique des lumières blanches instantées. *C. R. Soc. Biol. Paris*, 1891, **3** (9 sèrie), 601–604.
23. CHARPENTIER, A. Isolement des couleurs dans la luminière blanche par leur action successive. *C. R. Soc. Biol. Paris*, 1892, **4** (9 sèrie), 533–536.
24. COLENBRANDER, M. C. Über subjektive Farbenerscheinungen. *Acta ophthal., Kbh.*, 1933, **11**, 437–460.
25. DONISELLI, C. Sui fenomeni d'induzione cromatica da luce bianca e sulla natura dei processi consecutivi. *Arch. Fisiol.*, 1907, **4**, 561–593.
26. DOVE, H. W. Ueber Scheiben zur Darstellung subjectiver Farben. *Pogg. Ann. Physik u. Chemie*, 1848, **85** (151) 526–528.
27. DOVE, H. W. *Darstellung der Farbenlehre und Optische Studien*. Berlin: G. W. F. Müller, 1853. Pp. 281–283.
28. DUBOIS, R. Recherches expérimentales sur le rôle de la contractilité dans les mécanismes sensoriels chez les mollusques. *J. de Psychol.*, 1922, **17**, 787–805.
29. EDRIDGE-GREEN, F. W. The artificial spectrum top. *Nature, Lond.*, 1895, **51**, 321.
30. EDRIDGE-GREEN, F. W. Some subjective phenomena of vision. *J. Physiol.*, 1915–16, **50**, xl.
31. ERB, M. B., & DALLENBACH, K. M. 'Subjective' colors from line-patterns. *Amer. J. Psychol.*, 1939, **52**, 227–241.
32. EXNER, S. Bemerkungen über intermittirende Netzhautreizung. *Pflüg. arch. ges. Physiol.*, 1870, **3**, 214–240.
33. FECHNER, G. T. Ueber eine Scheibe zur Erzeugung subjectiver Farben. *Pogg. Ann. Physik. u. Chemie*, 1838, **45** (121), 227–232.
34. FINNEGAN, J. M., & MOORE, B. The

artificial spectrum top. *Nature, Lond.*, 1895, **51**, 292–293.

35. FORSTER, G. La théorie dynamique de la vision des couleurs. *Année psychol.*, 1923, **24**, 26–69.
36. FRY, G. A. Modulation of the optic nerve-current as a basis for color-vision. *Amer. J. Psychol.*, 1933, **45**, 488–492.
37. FRY, G. A. Color phenomena from adjacent retinal areas for different temporal patterns of intermittent white light. *Amer. J. Psychol.*, 1933, **45**, 714–721.
38. FRY, G. A. Color sensations produced by intermittent white light and the three-component theory of color-vision. *Amer. J. Psychol.*, 1935, **47**, 464–469.
39. FRY, G. A. Color sensations produced by intermittent spectral stimuli. *Amer. J. Psychol.*, 1936, **48**, 326–330.
40. GEBHARD, J. W. Chromatic phenomena produced by intermittent stimulation of the retina. *J. exp. Psychol.*, 1943, **33**, 387–406.
41. H., E. Expériences sur les transformations subjectives des couleurs. *La Nature*, 1897, No. 1268, 256.
42. HANNAY, J. B. Colour perception. *Nature, Lond.*, 1881–82, **25**, 604–605.
43. HELMHOLTZ, H.v. *Treatise on physiological optics.* (Vol. II.) (3rd ed.) (Ed., J. P. C. Southall). Menasha, Wis.; George Banta Publishing Co. for the Optical Society of America, 1924. Pp. 255–261.
44. HENRY, C. Applications à la tachymétrie et à l'ophtalmologie d'un mode de production, jusqu'ici inexpliqué, de la couleur. *C. R. Acad. Sci., Paris*, 1896, **122**, 406–408.
45. HURST, C. H. A spectrum top and a complementary one. *Nature, Lond.*, 1895, **51**, 510.
46. IVES, H. E. Visual diffusivity. *Philos. Magazine*, 1917, **33** (series 6), 18–33.
47. IVES, H. E. The resolution of mixed colours by differential visual diffusivity. *Philos. Magazine*, 1918, **35** (series 6), 413–421.
48. JUDD, D. B. Color vision. In O. Glasser (Ed.), *Medical Physics.* Chicago: Year Book Pub., 1944. Pp. 265–282.
49. KLOPSTEG, P. E. A curious color phenomenon. *Science*, 1931, **73** (new series), 590.
50. KORMANN, F. Die Erzeugung eines kontinuierlichen Spektrums auf Grund der Benhamschen Farben. *Z. Sinnesphysiol.*, 1931, **62**, 158–166.
51. LEMMON, V. W. Entoptic colors. *Science*, 1932, **75** (new series), 217.
52. LIVEING, G. D. On Benham's artificial spectrum top. *Nature, Lond.*, 1894, **51**, 167.
53. LIVEING, G. D. The artificial spectrum top. *Nature, Lond.*, 1894, **51**, 200.
54. LUCKIESH, M., & MOSS, F. K. A demonstrational test of vision. *Amer. J. Psychol.*, 1933, **45**, 135–139.
55. NEWTON & Co. The artificial spectrum top. *Nature, Lond.*, 1895, **51**, 463.
56. PAULI, R., & WENZL, A. Über Farbenempfindungen bei intermittierendem farblosen Lichte. *Arch. ges. Psychol.*, 1924, **48**, 470–484.
57. PERCIVAL, A.S. Colour phenomena due to intermittent stimulation with light: note on the colours of Benham's top. *Trans. ophthal. Soc. U.K.*, 1909, **29**, 119–125.
58. PIÉRON, H. Le méchanisme d'apparition des couleurs subjectives de Fechner-Benham. *Année psychol.*, 1922, **23**, 1–49.
59. PIÉRON, H. Des lois du déséquilibre chromatique initial et de la prépondérance de la diffusion chromatique dans l'excitation lumineuse de la rétine. (Méchanisme de production des couleurs subjectives de Fechner-Benham). *C. R. Soc. Biol. Paris*, 1922, **86**, 922–925.
60. PIÉRON, H. Le mécanisme des couleurs subjectives de Fechner-

Benham. *J. de Psychol.*, 1923, **20**, 75–80.

61. PIÉRON, H. Influence de la composition de la luminière sur la nature des couleurs subjectives de Fechner-Benham. Données complémentaires. *Année psychol.*, 1928, **29**, 229–233.

62. PREVOST, B. Sur une apparence de décomposition de la lumière blanche par le mouvement du corps qui la réfléchit. *Mémoires de la Société de Physique et d'Histoire naturelle de Genève*, 1823–26, **3**, 121.

63. ROOD, O. N. On a new theory of light, proposed by John Smith. *Amer. J. Sci. and Arts*, 1860, **30** (2nd series), 182–186.

64. R(OOD), O. N. Subjective optical phenomena. *Amer. J. Sci. and Arts*, 1861, **31** (2nd series), 417.

65. SANFORD, E. C. A laboratory course in physiological psychology: IV. *Amer. J. Psychol.*, 1893, **5**, 390–415.

66. SCHWARTZE, T. *Grundgesetze der Molekularphysik.* Leipzig: J. J. Weber, 1896. Pp. 200–206.

67. SEGAL, J. Une nouvelle manifestation de couleurs subjectives. *C. R. Soc. Biol. Paris*, 1940, **133**, 259–261.

68. SEXTON, F. P. The spectrum top. *Phys. Soc. Lond.*, 1907–08, **21**, 392–395.

69. SILBERSTEIN, L. Investigations on the intrinsic properties of the color domain. *J. opt. Soc. Amer.*, 1938, **28**, 63–85.

70. SILBERSTEIN, L. Investigations on the intrinsic properties of the color domain. II. *J. opt. Soc. Amer.*, 1943, **33**, 1–10.

71. SKINNER, B. F. A paradoxical color effect. *J. gen. Psychol.*, 1932, **7**, 481–482.

72. SMITH, F. J. Apparent decomposition of sunlight by intermittent reflecting surfaces. *Nature, Lond.*, 1881, **24**, 140.

73. SMITH, J. On the production of colour and the theory of light. *Rep. Brit. Ass. Adv. Sci.* (Notices and Abstracts), 1859, **29**, 22–23.

74. SMITH, J. On the cause of colour and the theory of light. *Amer. J. Sci. and Arts*, 1860, **29** (2nd series), 276–278.

75. SMITH, N. Colour perception. *Nature, Lond.*, 1882, **26**, 30–31.

76. STEWART, G. N. Is the law of Talbot true for very rapidly intermittent light? *Proc. roy. Soc. Edinb.*, 1887–88, **15**, 441–464.

77. STEWART, G. N. Color phenomena caused by intermittent stimulation with white light. *Amer. J. Physiol.*, 1924, **69**, 337–353.

78. TROLAND, L. T. The enigma of color vision. *Amer. J. physiol. Opt.*, 1921, **2**, 23–48.

79. TROLAND, L. T. *The principles of psychophysiology* (Vol. I). New York: D. van Nostrand Co., 1930. P. 250.

80. TURNER, D. The spectrum top. *Nature, Lond.*, 1895, **51**, 438.

81. VON KRIES, J. In H.v. Helmholtz, *Treatise on physiological optics.* (Vol. II) (3rd ed.) (Ed. J. P. C. Southall). Menasha, Wis., George Banta Publishing Co. for the Optical Society of America, 1924. Pp. 449–450.

82. WAY, E. F. Entoptic colors. *Science*, 1932, **75** (new series), 81.

83. WEYL, H. *Mind and nature.* Philadelphia: University of Pennsylvania Press, 1934, Pp. 1–19.

84. *Anon.* An "artificial spectrum top." *Nature, Lond.*, 1894, **51**, 113–114.

85. *Anon.* An artificial spectrum. *Scientific American*, 1895, **72**, 309–310.

86. *Anon.* The "artificial spectrum" or new colour top. *Engineering*, 1895, **59**, 381.

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AN EXTENSION OF CONTROL GROUP DESIGN

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This paper presents an elaboration of existing experimental design in cases where an experimental group and a control group are usually employed. It is intended as a recommendation to psychologists, especially those working in these specific fields: (1) transfer of training experiments; (2) experiments on induced changes in existing attitudes, opinions, and personal values; and (3) experiments on the effects of controlled experience on responses, skills, and performances already existing in the behavior repertoire. We feel that the use of the recommended design and procedures will provide information of a valuable sort, information which cannot be obtained with the use of existing standard research designs.

Brief History

The history of the idea of a controlled experiment is a long one. Usually one goes back to J. S. Mill's canons (**10**) for the concept of experimental controls. Those readers interested in the various types of controls which have been suggested in the history of psychological research may be referred to Fechner (**6**), Henri (**9**), Wundt (**21**), Titchener (**17**), Baldwin (**1**), and Warren (**18**). However, if one is interested solely in the concept of *Control Group* in the history of psychology, Twentieth Century psychology contains that history. We have not been able to find a single case of the use of control group design, as we use it today, before the year 1901. Control group design seems to have awaited the development of statistical concepts which allow for the characterization of group performances in terms of measures of central tendency; and psychologists seem to have been slow to combine statistical sophistication with experimental design. We were struck with the fact that the use of an experimental group and a control group in the ~~three~~ actual types of experiments cited above is a very recent development. The earliest use of this kind of experimental design in the study of transfer of training is found in the work of Thorndike and Woodworth (**16**), published in

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1901. These investigators used independent educational groups as controls on other groups. They were studying the influence of various educational sequences on proficiency in various subjects. In their observations a sequence of training such as *ABC* could be evaluated against a control group with sequence *ADC*. They attributed any differences in performance in *C* in terms of differential effects of *B* and *D*. Thorndike and Woodworth were examining educational sequences as they found them. In the actual experimental manipulation of groups, in transfer of training experiments, it is interesting that control group methodology was not being used at the time that Thorndike and Woodworth published their accounts. For example, we find a typical experiment in transfer of training by Ebert and Meumann (**4**) in 1904 in which one group was used, the typical experimental group receiving this sequence; pre-test, training, post-test. This procedure was criticized by Dearborn (**3**) in 1909, and by Sleight (**14**) in 1911. That the criticism did not immediately take hold is evidenced by the fact that Ewert (**5**) in 1926 repeated the criticism, and showed why a control group was necessary. It is interesting that the first thorough transfer of training experiment using two groups seems to have been performed in 1908 by Winch (**19**). Winch used the design which is standard today; his experimental group received pre-test, training, and post-test, while the control group received the pre-test, no training, and the post-test. It is curious that Winch did not consider his design as an innovation, and in fact, did not mention it as something special when he discussed his experiment. It seems safe to assume that prior to 1900 the experiments on transfer of training were subject to Ewert's criticism. He argued that without a control group the effects of taking a pre-test are not analyzable, and that the effects of the training procedures cannot be adequately evaluated. This point has been repeated many times (see Woodworth, **20**). Examples of experiments which have been criticized on these grounds are those of Swift (**15**), Scripture, Smith, and Brown (**13**), and Ebert and Meumann (**4**), along with many others. As far as terminology goes, we find that in the middle 'twenties Ewert (**5**) and Bray (**2**) freely used the term "Control Group" for the group that received no training. Warren included this term in his Dictionary (**18**) in 1934. Yet Harriman's recent New Dictionary (**8**) did not include it. If one is still skeptical about the relative newness of the whole idea, an examination of representative psychological textbooks serves as a check. We find that the concept of the control group generally made its way into these publications between 1920 and 1930. The idea of co-twin control and litter-mate control groups entered between 1930 and 1940.

The experimental studies on changes of attitudes, opinions, and personal values have not taken much advantage of the lessons in experimental design stemming from transfer of training experiments. The common procedure has been to give a group a pre-test, using an acceptable attitude scale, then subject the group to educational procedures of some sort, and then post-test the group with the same test or an equivalent form of it. Without a control group it is impossible to evaluate either the effects of the educational procedure or the influence of taking the attitude scale. The experiment of Peterson and Thurstone (**12**) on the effects of motion pictures upon the attitudes of children represented the first serious attempt to use control groups for the purposes of evaluating the effects of the educational procedures. This was published in 1933. Murphy, Murphy, and Newcomb (**11**, pp. 946–979) in their summary of experiments on the modification of attitudes, listed many recent experiments where no control group was used.

In general, the arguments for the use of a control group apply in the same way to the three fields of endeavor listed in our Introduction. Woodworth (**20**) has included an exhaustive account of experimental design in his discussion of transfer of training experiments. More recently, Gagné, Foster, and Crowley (**7**) have reviewed this problem of experimental design in the measurement of transfer effects. Many of their conclusions apply to experiments on induced attitude changes and the acquisition of skills. To review their recent article is unnecessary; the designs for experiments are thoroughly outlined there. The important point we wish to make is that their recommendations involved the use of an experimental group which receives controlled treatment and a control group which does not. For the three types of experiment we shall discuss, the current standard designs may be tabulated as follows:

(1) *Transfer of training:*

	Experimental Group	*Control Group*
Pre-test on Task_1	yes	yes
Training on Task_2	yes	no
Post-test on Task_1	yes	yes

(2) *Experimentally induced changes in attitudes, opinions, or values:*

	Experimental Group	*Control Group*
Pre-test on Attitude_1	yes	yes
Education, Propaganda	yes	no
Post-test on Attitude_1	yes	yes

(3) *The effects of controlled experience on any existing behavior:*

	Experimental Group	Control Group
Pre-test on Present Behavior$_1$	yes	yes
Experimental Treatment	yes	no
Post-test on Final Behavior$_1$	yes	yes

With matched groups, or with matched tasks, both Woodworth (20) and Gagné, Foster, and Crowley (7) listed appropriate substitute designs, but all are based upon an experimental and a control group. It is understood that the two groups can be matched by one criterion or another or may be randomly drawn from the population. The pre-test and post-test do not have to be identical tests, but they can be equivalent forms based on standardization procedures, or can be equivalent in some logical manner. It is important to state that the same scale units should be involved in both the pre-test and post-test.

We have attempted to follow the history of control group methodology in a sketchy way, and have outlined what present practice seems to be. We now wish to proceed to an extension of present practice to what we believe is a more adequate design of experimentation.

An Extension of Experimental Design

The experimental plan outlined above possesses these possibilities for analysis: (1) assignment of a numerical value for the effects of taking the pre-test as this factor operates on the post-test scores; (2) assignment of a numerical value for the effects of the training on the magnitude of the post-test score; and (3) improvement scores based on the contribution of the training alone and the taking of the pre-test. An assumption of additivity of scores is involved here. We wish to point this out, but we feel that it is inappropriate to go into all of the ramifications of the theory of scales of measurement. The assumption of additivity allows us this privilege: the improvement score for the control group may be subtracted from the improvement score of the experimental group in order to obtain a numerical value for improvement due to *training procedure alone.* There is at present no real agreement on the permissibility of this procedure. All we can say is that the information it gives often proves useful. This procedure is discussed by Gagné, Foster and Crowley (7).

It was decided ~~We have seen~~ that current control group design will allow us to assign numerical values for the effects of *taking the pre-test alone* and the effects of *training alone.* The main point ~~we wish to make~~ made is that these two items of information are still inadequate to characterize what hap-

pens in many experiments of the sorts we have discussed. We feel that the pre-test operates directly upon the effectiveness of training, or *interacts* with the training process. That is, there is a great possibility that merely taking a pre-test changes the subjects' attitudes toward the training procedures. Or it may conceivably change the "set" or attentional factors important to the effectiveness of training. Thirdly, it may actually change the manner in which the subjects "perceive" the training material. These psychological possibilities may be termed "interactions." Thus, if a group of subjects were given a pre-test on spelling ability and the test list contained a lot of *ie* and *ei* vowel pairs, it is perfectly conceivable that the subjects would "pay attention to" certain relevant portions of a spelling lesson in a manner very different than if they had not received the pre-test at all. Or, in the case of attitude experiments, the way that the pre-test items are worded might influence the manner in which the propaganda or education procedures are "received" by the subjects. We believe that this kind of pre-test-training interaction is important, and that it occurs in a large number of experiments of the variety we have listed above. We have found that these interaction effects can be quantified. The following control group design is necessary to do it:

	Experimental Group	*Control Group I*	*Control Group II*
Pre-test	yes	yes	no
Training, Education	yes	no	yes
Post-test	yes	yes	yes

The question immediately arises concerning the possibility of obtaining an improvement (or change) score for control group II. This, we believe, may be legitimately done by statistical inference, provided that: (1) the groups are either reasonably matched at the beginning of the experiment on the basis of as many criteria as possible or are randomly drawn from the population; (2) the means and variances for the experimental group and the control group I are known for the pre-test; and (3) the *N*'s for all three groups are reliably large. This latter qualification is, of course, partly dependent upon the prior experience of the experimenter with performances of samples on the task in question. If we take these three provisions into account, we can infer what the pre-test score for control group II *would have been* had this group actually taken the pre-test. The grand mean pre-test score for the experimental group and control group I (pooled data) represents the best estimate of what the mean of the control group II would have been had this group taken the pre-test. We shall call this value "*i*", the "inferred pre-

test score" for control group II. Table I outlines the numerical values which must be fitted into the analysis in order for the interaction effects to be quantified.

TABLE I

NUMERICAL VALUES NEEDED FOR THE ANALYSIS OF INTERACTION EFFECTS IN A THREE-GROUP DESIGN

	Experimental Group	*Control Group I*	*Control Group II*
Pre-test Means	$a_1 \pm \sigma_{ma_1}(P_1)$	$a_2 \pm \sigma_{ma_2}(P_1)$	$i = \frac{a_1 + a_2}{2} \pm \sigma_{ma_{1,2}}$
Training	yes (T_1)	no	yes (T_1)
Post-test	$b_1 \pm \sigma_{mb_1}$	$b_2 \pm \sigma_{mb_2}$	$b_3 \pm \sigma_{mb_3}$
Improvement or Change in Means	$d_1 = b_1 - a_1$	$d_2 = b_2 - a_2$	$d_3 = b_3 - i$

Note that the inferred pre-test score, i, involves an inferred standard error of the mean obtained from the first two groups. The implications of the table above may be outlined as follows: (1) $d_3 = f(T_1)$, or, the difference score for control group II is a function of the training process *alone;* (2) $d_2 = f(P_1)$, or, the difference score for control group I is a function of the pre-test process *alone*, and (3) $d_1 = f(T_1 + P_1 + I)$, or the difference score for the experimental group is a function of the effects of taking the pre-test, the effects of the training period, as well as of the interaction effects existing between the pre-test and training processes. This relationship is assumed to be additive. Here, I is the term to which we have been referring as *interaction*, or possibly, attentional, perceptual shifts due to taking the pre-test. The term I may be positive or negative, depending upon the psychological effects of the pre-test on the way the subject "approaches" the training.[3]

In summary, with reference to Table I what we have done is to assume that: (1) the improvement of control group II is due to training or education procedures alone; (2) the improvement of control group I is due to facilitating effects of the pre-test alone; and (3) the failure

[3] There are other interactions possible, such as the training-post-test, and pre-test-post-test interactions. We shall omit such considerations here; this is equivalent to the assumption that the post-test is a measuring device of a "pure" sort, involving no learning due to its use.

of the improvement of the experimental group to be equal to the sum of the improvement scores of the two control groups is a reflection of interaction effects. These effects are quantified as follows: $I = d_1 - (d_2 + d_3)$.

There are many ways to interpret an obtained interaction. For example, if an interaction term is relatively large compared with the improvement of the experimental group, and if it is positive in sign, we might assume that the pre-test somehow facilitated the effective use of training procedures. We then might start on a search for the kinds of attitudinal and perceptual changes that the pre-test had produced, since such changes might have a generality of application for many kinds of educational procedures. In attitude testing, a negative I term would lead one to suspect that the pre-test developed some kind of resistance to the propaganda material. This problem then could be approached by new experiments. It is possible that rules for building pre-tests which make subjects receptive to education might come out of such a program. In other words, the magnitude and direction of I ought to provide some hints to the experimenter.

We have indicated the various possible psychological statuses of the interaction term, I. There still remains the challenging problem concerning the psychological status of *training alone*. It is hard to conceive of a change in behavior due to training alone. Life history has produced certain attitudes which must inevitably interact with training procedures. The problem of familiarity is important here, together with the total personality organization. In order to treat logically the concept of *training alone* it seems necessary to postulate some prior state, A, which is common to all three groups at the time of the pre-test administration. Then training may be inferred to interact with A under all conditions in the experiment. This construction enables us to state that, all other things being equal, the improvement of control group II is due to training alone or to the training-A interaction, which ever one might prefer. Parenthetically it might be added that a study of covariance during successive stages in the training procedure might reveal some of the training-A interactions. Much work needs to be done in elucidating this problem.

Rather than indulge in the listing of all of the permutations and combinations of outcomes possible within Table I let us examine a small demonstration experiment which we have completed in order to portray one use to which the three-group design may be put.

A Sample Experiment, Three-Group Design

Two grammar school classes, a fifth and a sixth grade, were used in this experiment. The pupils in each class were assigned to the three

groups required in the new design. The groups were roughly equated in spelling ability by means of teachers' judgments. The experimental group was pre-tested on a list of words of equal difficulty. Control group I was pre-tested at the same time on the same list of words. Control group II was out of the rooms at the time. Then the experimental group and control group II were given a standard spelling lesson, covering some general rules of spelling. Control Group I was out of the classrooms during this training period. Then the post-test was given to all of the groups. It was composed of the same words as in the pre-test. The results for the two grades are presented in the tables below. It is regrettable that the N for each group was not larger, but in order to administer this experiment properly it was necessary to restrict the sample size. If we consider the sixth grade experiment to be a replication of the fifth grade experiment, the small sample size does not loom so importantly. It is to be noted that the variabilities are not inordinately large. We are not satisfied with the sample size, but we are willing to consider this experiment to be representative, highly indicative, and capable of reproduction.[4]

The I term in Table II is acknowledged to be but a "best estimate.". But an examination of the variabilities is enough to reassure one that the chances are very small that the population I is zero. Added assurance may be obtained by an examination of the sixth grade results in Table III.

TABLE II

SUMMARY OF FIFTH GRADE RESULTS OF AN EXPERIMENT IN SPELLING, USING A THREE-GROUP DESIGN

	Experimental Group ($N=10$)	*Control Group I* ($N=10$)	*Control Group II* ($N=10$)
Pre-test	yes (P_1)	yes (P_1)	no
Pre-test means	$a_1=3.2\pm0.8$	$a_2=2.8\pm0.7$	$i=3.0\pm0.6=\frac{1}{2}(a_1+a_2)$
Training	yes (T_1)	no	yes (T_1)
Post-test	yes	yes	yes
Post-test means	$b_1=9.9\pm1.6$	$b_2=3.5\pm0.8$	$b_3=11.2\pm1.2$
Improvement means	$d_1=6.7\pm0.9$	$d_2=0.7\pm0.5$	$d_3=8.2\pm?$
Value of I	$d_1-(d_2+d_3)=-2.2$		

[4] This experiment was carried out with the aid of Mr. Richard Walk, Harvard University. We are greatly indebted to Miss Mildred March, principal of the John Ward School, Newton, Massachusetts, for her helpful cooperation, and to Miss Anne Wilson and Miss Cecilia Dunlop, teachers, for indispensable aid in carrying out the experiment.

TABLE III

SUMMARY OF SIXTH GRADE RESULTS OF AN EXPERIMENT IN SPELLING, USING A THREE-GROUP DESIGN

	Experimental Group ($N=8$)	*Control Group I* ($N=9$)	*Control Group II* ($N=8$)
Pre-test	yes (P_1)	yes (P_1)	no
Pre-test means	$a_1=5.4\pm1.4$	$a_2=6.0\pm1.5$	$i=5.7\pm0.8=\frac{1}{2}(a_1+a_2)$
Training	yes (T_1)	no	yes (T_1)
Post-test	yes	yes	yes
Post-test means	$b_1=11.8\pm1.0$	$b_2=6.8\pm1.4$	$b_3=14.4\pm0.3$
Improvement means	$d_1=6.4\pm1.2$	$d_2=0.8\pm0.6$	$d_3=8.7\pm?$
Value of I	$d_1-(d_2+d_3)=-3.1$		

Again, we obtain a sizeable interaction term with negative sign. We have little reason to believe that this is an accidental occurrence when we consider that the fifth grade results are comparable. The interpretation of the negative interaction term offers interesting possibilities. We know that the taking of the pre-test somehow diminished the effectiveness of the training in spelling. One possibility is that the pre-test was emotionally disturbing to the children. Another possibility is that the writing down of a word erroneously on the pre-test may have produced perseverative errors. An examination of our data does not reveal this to be true for the experimental group in excess of control group I, but we have no standards against which to evaluate the occurrence of perseverative errors.

One fact stands out clearly. If we had used the ordinary two-group design (the experimental group and control group I) the effectiveness of the teaching procedures would have been erroneously underrated. The pre-test has been shown to have vitiated some of the effectiveness of the teaching method. Thus we have revealed a paradoxical case: ***with the currently-used two-group design the method used to evaluate the effectiveness of training procedures actually may operate to cut down the effects of training as they are capable of being measured by ordinary procedures.*** Using control group II, however, allows us to evaluate more adequately the effects of training or the success of the teaching method. The implications here for educational research seem obvious.

THE USE OF A FOUR-GROUP DESIGN

Other experiments using the three-group design are in progress in the Laboratory of Social Relations, Harvard University, placing special

emphasis on the problem of experimentally-induced attitude changes. Preliminary experiments have revealed that large interactions often appear in such experiments, thus demonstrating the need for control group II. In addition, the mathematical implications of the three-group method are being explored. The analysis of data presented above was the simplest possible for the purpose. It seems clear that more sophisticated statistical procedures, such as an adaptation of the analysis of covariance, may enhance the evaluation of research results. In particular, the mathematical nature of I, the interaction term, needs to be investigated. Its meaning is at present obscure, and the reason for this obscurity may be revealed by Table IV.

TABLE IV

SUMMARY OF MEAN SCORES AVAILABLE FOR THE ISOLATION OF INTERACTION EFFECTS BETWEEN PRE-TEST AND TRAINING IN A THREE-GROUP DESIGN

		Pre-Test	
		yes	no
Training	yes	Exper. Group	Control II
	no	Control I	??

If this table above could be filled out logically with mean improvement scores it seems clear that a straight analysis of variance, with a solution for the interaction between pre-test and training, would be feasible. But a group with no training and no pre-test is not sensible at all, with reference to the laboratory experiments with which we have been dealing. The post-test score for such a group would be in effect a pre-test, and no improvement score could emerge. Thus, that I is a "true" interaction magnitude may be questioned for the case of carefully controlled laboratory experimentation.

However, in the case of field studies, such as those on attitude changes in the American soldier as a result of propaganda,[5] there is an opportunity to fill in the empty cell in the four-fold table, provided that we use a fourth group, control group III. In the case of such field studies we may consider that passage of time is an important variable, since many uncontrolled events may occur. For example, we may be giving the American soldier propaganda about the great strength of our ally, Russia. Sometime between the pre-test and the post-test, Russia

[5] We are indebted to Professor S. A. Stouffer for this information and for pointing out the necessity of a four-group design.

may lose an important battle to the Germans. This is an uncontrolled event, which we shall designate as E_1, taking place in the period of time between the pre-test and the post-test. This event is assumed to impinge upon all members of all four groups. The design of the experiment required to reveal interaction effects in such a field experiment is shown in Table V.

TABLE V

NUMERICAL VALUES NEEDED FOR THE ANALYSIS OF INTERACTION EFFECTS IN A FOUR-GROUP DESIGN

	Experimental Group	*Control Group I*	*Control Group II*	*Control Group III*
Pre-test	yes	yes	no	no
Pre-test means	$a_1 \pm \sigma_{ma_1}$	$a_2 \pm \sigma_{ma_2}$	$i = \frac{a_1+a_2}{2}$	$i = \frac{a_1+a_2}{2}$
Training	yes (T_1)	no	yes (T_1)	no
Event, E	yes (E_1)	yes (E_1)	yes (E_1)	yes (E_1)
Post-test	yes	yes	yes	yes
Post-test means	$b_1 \pm \sigma_{mb_1}$	$b_2 \pm \sigma_{mb_2}$	$b_3 \pm \sigma_{mb_3}$	$b_4 \pm \sigma_{mb_4}$
Change means	$d_1 = b_1 - a_1$	$d_2 = b_2 - a_2$	$d_3 = b_3 - i$	$d_4 = b_4 - i$
Interaction	$d_1 - (d_2 + d_3 - d_4)$			

It is to be noted that we now infer *two* pre-test means based on statistical assumptions: those for control groups II and III. Any change in score taking place in the time between the pre-test and post-test for group III would be attributed to the effects of event E_1. Since all of the groups are exposed to this event, the following statements may be made: (1) $d_4 = f(E_1)$, or, the change in attitude for group III is a function of the outside, uncontrolled event, E_1; (2) $d_3 = f(E_1 + T_1)$, or the change in attitude for control group II is a function of the training and the event, E_1; (3) $d_2 = f(E_1 + P_1)$, or the change in attitude for control group I is a function of the effects of the pre-test and the event E_1; (4) $d_1 = f(E_1 + P_1 + T_1 + I)$, or the change in attitude for the experimental group is due to the summative action of the pre-test, the event E_1, the training or education process, and the interaction between the pre-test and the succeeding events in time (E_1 and T_1). Admittedly, this analysis seems destined to be cumbersome under many circumstances. But it appears necessary in order to uncover and numerically weight the interaction phenomenon. We note now that our four-fold table (Table IV) above can be filled out when we use this design, by inserting control group III into the lower right-hand cell.

In view of our argument above, is it still impossible to fill in the fourth cell of the four-fold table *for the three-group design?* True, we left that table with some question marks in the fourth cell; but we believe in the light of an analysis of the four-group design, that it may be possible to do something about the missing cell. The most obvious step would be to consider the three-group design as indicative of measures *in time.* A group with no pre-test and no training could not vary with time insofar as our measures apply. We could therefore infer a zero improvement score for the fourth cell. This seems a fairly sound procedure in well-controlled laboratory experiments, but it is subject to these dangers: (1) an unknown event, *E*, may have affected our three groups, so that if we actually had a fourth group, a difference score would have appeared; (2) time passage, maturation, or growth processes may have made the obtained post-test score different from the "true" pre-test score. Actually, if these cases do occur, they require the four-group design in order to analyze adequately the experimental interaction. This means that many laboratory experiments in learning, motivation, nutrition, and maturation, have been inadequately designed. This is especially true of cases where some pre-condition of the animal has been measured in order to give the experimental measure a baseline. For example, in the lever-pressing experiment, we often obtain a sample of lever-pressing activity in a pre-test, to get a baseline for unrewarded behavior in this class. But these animals all grow older, and this can be called event *E*. Thus, in order to assess adequately the improvement or learning scores, a four-group design seems to be appropriate. We are presenting this argument for serious consideration. Actually, at present we are not certain that such elaborate control group design is absolutely necessary; but we suspect that it is, especially in cases where accurate quantification and the discovery of constants is the aim of the experimenter.

That the interaction term, *I*, may be of importance in evaluating research which falls within the three main areas we have considered above, seems evident. We would like to exhort psychologists who work in these fields to study further the uses of the three-group and four-group experimental designs and report the results for discussion. It seems possible that many previously unquantified effects—attentional, attitudinal, or perceptual, may be revealed and weighted by using the suggested plans. At least that is the hope we entertain on the basis of preliminary indications.[6]

[6] Since the time this manuscript was submitted, a noteworthy finding has been studied. We have preliminary evidence that the pre-test may operate to reduce post-test variance in studies of attitude change. This is evidence of another sort of interaction. It is, in effect, the limiting of variance by addition of restrictions of "attention."

Summary

We have presented an extension or modification of currently-used control group design. We have shown that the use of a three-group design, and in some cases a four-group design, has potentialities for revealing and weighting certain interaction effects. These interaction effects may shed light on attentional, attitudinal, and perceptual factors which often are important in these three types of psychological experimentation: (1) transfer of training; (2) induced changes in attitudes, opinions, and values; and (3) the effects of controlled experience on responses, skills, or performances which already exist in the behavior repertoire. Further study of the extension in experimental design is recommended.

BIBLIOGRAPHY

1. Baldwin, J. M. *Dictionary of philosophy and psychology.* New York: Peter Smith, 1901. (See p. 229, Vol. I.)
2. Bray, C. W. Transfer of training. *J. exper. Psychol.*, 1928, **11**, 443–467.
3. Dearborn, W. F. The general effects of spaced practice on memory. *Psychol. Bull.*, 1909, **6**, 44. (Abstract.)
4. Ebert E., & Meumann, E. Über einige Grundfragen der Psychologie der Übungsphänomene im Bereiche des Gedächtnisses. *Archiv. f. ges. Psychol.*, 1904, **4**, 1–232.
5. Ewert, P. H. Bilateral transfer in mirror-drawing. *Ped. Sem.*, 1926, **33**, 235–249.
6. Fechner, G. T. *Elemente der Psychophysik.* Leipzig: Breitkopf und Härtel, 1889. (Pp. 76–87.)
7. Gagné, R. M., Foster, H., & Crowley, M. E. The measurement of transfer of training. *Psychol. Bull.*, 1948, **45**, 97–130.
8. Harriman, P. L. *The new dictionary of psychology.* New York: Philosophical Library, 1947. (See p. 87—no mention of control group.)
9. Henri, V. Revue générale sur le sens du bien de la peau. *L'année psychol.*, 1895, **2**, 295–362.
10. Mill, J. S. *Logic.* Bk. III, chap. 8, sec. 2.
11. Murphy, G., Murphy, L. B., & Newcomb, T. M. *Experimental social psychology.* New York: Harper, 1937.
12. Peterson, R. C., & Thurstone, L. L. *The effect of motion pictures on the social attitudes of high school children.* 1933.
13. Scripture, E. W., Smith, & Brown. On the education of muscular control and poser. *Studies from the Yale Psychol. Lab.*, 1894, **2**, 114–124. (Original not seen.)
14. Sleight, W. G. Memory and formal training. *Brit. J. Psychol.*, 1911, **4**, 386–457.
15. Swift, E. J. Studies in the psychology and physiology of learning. *Amer. J. Psychol.*, 1903, **19**, 201–220.
16. Thorndike, E. L., & Woodworth, R. S. The influence of improvement in one mental function upon the efficiency of other functions. *Psychol. Rev.*, 1903, **8**, 247–261, 384–395, 553–564.
17. Tichener, E. B. *Experimental psychology.* Vol. II. Quantitative experiments. Part II. Instructors Manual. New York: Macmillan, 1923. (See p. 340.)

18. WARREN, H. C. *Dictionary of psychology*. Boston: Houghton Mifflin, 1934. (See p. 60.)
19. WINCH, W. H. The transfer of improvement of memory in school-children. *Brit. J. Psychol.*, 1908, **2**, 284–293.
20. WOODWORTH, R. S. *Experimental psychology*. New York: Holt, 1938. (See pp. 178–181.)
21. WUNDT, W. *Grundzüge der physiologischen Psychologie*. Leipzig: W. Englemann, 1911. (See p. 399, Vol. III.)

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ANIMAL HYPNOSIS

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Despite the fact that the induced immobilization of animals has scarcely been dealt with in English psychological literature and is virtually extinct as an area of experimental investigation, the problem has had a protracted German-French history. Volgyesi (50) claims that the first reference to immobilization was made in 1636 by Daniel Schwenter, a mathematics professor at the University of Altdorf. Most authors, however, date its inception as an investigated phenomenon at 1646, with the appearance of the "Experimentum mirabile de imaginatione gallinae Kircher" (50). Kircher indicated that, by holding a hen in an uncomfortable position and fixing its head, a state of entrancement could be induced when a chalk line was drawn from its beak outward. Astounded by the "miraculous" nature of the phenomenon, Kircher concluded that the state stemmed from the hen's imagination. Despite the novelty of the discovery, enthusiasm for the solution of the problem failed to find additional fertile ground, and the problem remained in obscurity.

Development in the Nineteenth and Twentieth Centuries

Not until the middle of the nineteenth century did immobilization of animals again become a source of interest and investigation. Based on Faria's (50) notion of the hypnotic efficacy of visual fixation, some animals were said to entrance others by looking steadily into the eyes of their prey. The literature of the period reflects the influence of Mesmer's concept of "animal magnetism." Like Mesmer and Faria, the writers perpetuated the notion that some magnetic material emanated from the magnetizer to the magnetized by means of "passes." In 1834, Wilson wrote of the effect of "passes" of a magnetic nature made upon fish, cats, dogs, hens, pigs, geese, a horse, and a lioness. Describing the effect of these "passes" on a Bantam hen and cock, he says:

> The cock at first chuckled, and made much noise; then he became quiet, and remained so; sometimes sat down and closed his eyes; but towards the end he stood upright for a considerable time, like a statue, and neither moved hand nor foot; and when the cage was taken away, he moved not in the least, allowed me to touch and pull his comb and gills, and to stroke him down without making the slightest movement of his feet, head, or neck. . . . (52, p. 20).

In 1854, Newman mentioned the mesmerizing of birds and mice by snakes, and the taming of horses in half an hour by means of magnetic "passes." Describing the interest in animals at this time, Newman adds:

> Animals have been frequently fascinated for purposes of experiment, and a universal rigidity of the muscles produced to such an extent as to cause them to resemble pieces of statuary, so that the animal could be taken up and its whole weight supported by one foot and this state produced and continued at pleasure (**26**, pp. 16–17).

In 1881, Preyer (**30**) had credited Braid with the observation that a phenomenon similar to human hypnosis existed in animals in moments of extreme danger. This reaction Braid called "monoideismus," a combination of cataplexy, hypnosis, fascination, and all other phenomena which temporarily disturbed the nervous system because of the limitations imposed on it by a focused attention. Braid cited the attraction of the bird to the staring snake as an example of monodeismic activity based on a single, dominant, attention-compelling stimulus. Preyer, however, emphasized cataplexy (a fear reaction) and is generally credited as proposing the idea of "fear" as a possible explanation of the condition.

With the existence of the phenomenon firmly established in many forms of animal life, the literature (**38**, **48**) now dealt with two principal aspects of the problem: the characteristics of the trance, and the nature and function of the state itself. The predominant explanations of the function or nature of the state were: the instinct to simulate death for protective purposes, hypnotism, or the paralysis of fear (cataplexy).

In the twentieth century the work of Holmes (**17**, **18**, **19**) Hoagland (**14**, **15**) and, to a lesser extent, Mowrer (**25**) and Foley (**10**) have helped to make a mark on the American psychological scene. Although the investigation in the United States has continued to be sparse, several studies have been reported in which an immobile state has played a secondary role. Boas and Landauer (**2**) note, for example, that the best readings for cardiac rate in fowls were obtained by turning the animals on their backs. They would remain in this position for 20 to 30 minutes at a time, and sometimes for as long as two hours. Recordings were then made in these inactive states. Marcuse and Moore likewise note that their tantrum pigs could be "quieted" for conditioning studies. Describing the agitation of the pig in the Pavlovian frame, the authors write:

> It was possible to quiet the animal completely merely by scratching or clipping her flank with a pair of scissors. The quieting effect was almost instantaneous. The grunting and struggling ceased, her eyes closed, her breathing became more regular and her whole body relaxed, often to the point of hanging limply in the

straps. Cessation of the clipping immediately resulted in a full-scale resurgence of "tantrum" behavior (22, pp. 236–237).

Stroder (**43**) notes, in a study of narcosis in frogs, that the narcotization time is shortened by turning the subject on its back. Minami and Dallenbach (**23**) made use of the immobilization technique to compare "quiet" and active periods in a study of retroactive inhibition in the cockroach.

The designation of the phenomenon. In a historical survey of the literature one finds that the name of the condition has varied considerably. It has been referred to in the earlier literature as mesmerism (**52**), and fascination or entrancement (**26**). Darwin (**8**) called it a death-feint, while others used the terms cataplexy (**30**), conscious simulation of death (**4**), and immobilization reflex (**37**). Minami and Dallenbach (**23**) called the condition akinesis, while Mangold, earlier, had called it hypnotic akinesis. The state has also been referred to as hypertonicity (**30**), tonic immobility (**10**, **14**, **15**), myotonia congenita (**20**, **44**,) inhibitory state (**23**), or simply, inhibition (**11**, **28**).

By 1938, Stroder (**43**) had written that over 100 works were concerned with the designation of the phenomenon. Steiniger (**40**), in one of the more recent papers on this problem, cites 14 such terms and attempts to distinguish between their possible meanings.

The Induction of the Trance

Common denominators or differential factors in the types of inducing stimuli have not been systematically dealt with, and one finds scattered through the literature references to a variety of methods employed by the respective experimenters. The earliest method, introduced by Kircher (**24**), consisted of holding the hen in an unusual position, with its head held down on a flat surface while a chalk line was drawn from its beak outwards. The chalk line was later shown to be an extraneous factor.

Carrington (**5**) lists three principles of induction: bodily posture, fixation of the eye, and pressure on certain nerve centers. Dearborn (**9**) considers the nature of the inducing stimulus to be tactile or visual. Hoagland (**15**) names stroking between the eyes, pressure on the thorax, or sudden dorsal position. Rabaud (**31**), on the other hand, claims that immobility is never invoked by a sensory influence, but by a sudden shock or concussion.

Foley (**10**) classified the methods into the following four principal categories:

Repetitive stimuli. The use of this method involved the stroking of

body parts such as might have been used in mesmeric "passes." Mowrer (25), for example, describes stroking the comb of the rooster until it became quiet. Bonnet and Saboul (3) mention scratching the area around a frog's eyes, pinching its nostrils, or closing its eyes. Repetitive methods have sometimes consisted of "gazing" into the eyes of the animal in the fashion initiated by Braid (50), or by the sudden presentation of a light stimulus. Mowrer (25) mentioned that the sudden presentation of strong photographic lights accidentally induced immobility in his rooster, an incident which first seems to have stimulated his interest in the problem.

Another method which might be considered repetitive is the "swing" technique where the animal is generally held in the hands of the experimenter and swung gently back and forth several times (50, 51). When the animal is set down, it is usually immobilized. Parker tells of suspending a sea anemone from a hook and enclosing it in a glass cage.

> The contracted condition can be easily induced by moving the glass vessel in which the sea anemone is suspended back and forth a little, whereby the sea anemone is made to swing and strike on the sides of the vessel. By thus stimulating the sea anemone from time to time, it can be kept in a condition of continuous and extreme contraction (27, p. 51).

Marcuse and Moore (22) also made use of a repetitive technique via continuous "stroking" or scratching at the hide of a pig.

Pressure on body parts. Some investigators felt that exerting pressure on a particular part of the body, the precise part differing with the species, would induce the state (31). Minami and Dallenbach (23) induced "rest" in their cockroaches by pressing a finger lightly against the abdominal area of the inverted subject.

Inversion. Other investigators have utilized the technique of sudden inversion. Boas and Landauer (2) mention turning the hens on their backs to keep them quiet during cardiac recordings. Rode (37) turned owls on their backs to induce the immobility, while Reisinger (34) achieved the condition in this way with Indian locusts. Stroder (43) applied this method to frogs.

Restraint from movement. Although Foley cites this as only one of several methods, some degree of restraint seems to have been present in all the investigations. Marcuse and Moore (22) had their pigs restricted in a Pavlovian frame. Mowrer (25) held the rooster's legs, while stroking his comb. Reisinger (34), working with crayfish, held the head and claws motionless, or restricted movement by holding the tail. Pavlov (28) restrained his dogs by the straps of the conditioning apparatus employed. Dearborn (9) restricted his animals from movement until efforts to move ceased. Ten Cate (46) held the octopus in his hand to prevent its moving. It is not difficult to infer from the work of Bonnet and Saboul (3), that to scratch or stroke any part of the frog, the animal must have been held down, thus restricting spontaneous

movement. Minami and Dallenbach (**23**), by exerting pressure on the insects, in effect, produced restriction of movement. Gilman, Marcuse and Moore (**12**) immobilized chickens by restraining movement of the head and limbs. Inverting an animal, or holding it in an unusual posture, would also produce restriction of movement. Similarly cases where "visual fixation" was thought to be effective are to be understood in terms of the physical restriction needed to permit "gazing." Steiniger (**42**) cited an example of this when he describes Volgyesi's use of the fixation method to immobilize a lion. Volgyesi sat on the back of the lion, placing his hands on both sides of the animal's head and pulling it back so their eyes could meet. Moll's reference to Kircher's original experiment gives a more detailed and informative description of the chalk-line, visual-fixation method.

A hen is held down on the ground; the head in particular is pressed down. A chalk-line is then drawn on the ground, starting from the hen's beak. The hen will remain motionless. Kircher ascribed this to the hen's imagination; he said it imagined it was fastened, and consequently did not try to move. Mach mentions Kircher's experiment . . . as an example of the ease with which an experiment may be erroneously interpreted. For a long time the chalk-line was held to be the essential part of the experiment . . . however, it was shown that the experiment could be successfully performed by merely holding the hen down to the ground. . . . This much is certain: many animals will remain motionless in any position in which they have been held by force for some time . . . in general, the stimulus used with the animals is tactile. . . . (**24**, p. 200).

Rijlant, describing his use of Kircher's method, says:

L'animal est imobilisé sur une table et une ligne droite est tracée à la craie devant ses yeux . . . l'animal entre en état d'hypnose et s'y maintient pendant des heures. (**35**, p. 417).

Characteristics of the Trance

Status of the musculature. Some observations of the appearance of the phenomenon describe it as a tetanic muscle contraction (**4**, **6**, **15**, **21**, **30**), or atonicity (**1**, **45**, **46**). Most authors who have been concerned with the problem of muscle condition seem to believe that hypertonicity is characteristic of the condition. Foley says that " . . . a high degree of plastic tonus of the skeletal musculature is characteristic of tonic immobility" (**10**, p. 515). Dearborn observed that crayfish fall into a " . . . state of extreme tonicity amounting often almost to a cramp of the muscle" (**9**, p. 407). But it is also possible, as Rijlant (**35**) claims, that both a relaxed and contracted muscle condition may occur during the course of a trance. Rode (**37**) observed that the entire body of his owl contracted, and then relaxed slowly.

Heart rate. Although one finds some references to cardiac changes in the immobilized state, little of the material is quantitative. Preyer (**30**) and Verworn (**49**) observed that heart rate increased. Sidis (**39**),

however, claimed his frogs showed retarded cardiac rate. Beritoff (**1**), working with several vertebrates, also noted cardiac decrease, as did Marcuse and Moore (**22**), working with pigs. Rijlant (**36**), found no change in the heart rate of an immobilized rabbit. Gilman, Marcuse and Moore (**12**) working with chickens found no significant difference in the heart rate of susceptible animals during trance, nonsusceptible, and control animals.

Respiration. Prayer (**30**) referred to noisy respiration, while Verworn (**49**) and Coriat (**6**) observed accelerated respiration. Rode wrote:

> Dès que la Chevêche est "endormie" le fait le plus frappant est la modification du rhythme respiratoire qui s'accélère, alors que la cage thoracique se dilate beaucoup plus que pendant la respiration normale (**37**, p. 452).

Parker (**27**), in his study of the sea amenome, observed that the excretion of carbon dioxide was greater in the process of contraction than in the process of relaxing; however, no significant differences in output were observed during the contracted, as compared to the relaxed, state. Sidis (**39**), Piéron (**29**) and Beritoff (**1**) claim that respiration decreased, while Ten Cate mentions that respiration was unaffected (**46**).

Analgesia. Although the majority of experimenters believe analgesia to be present in the immobilized animal, there is no unanimity of opinion. Holmes (**18**) and Darwin (**8**) both describe the absence of pain-reaction in immobilized insects, while Stroder (**43**) mentions analgesia in immobilized frogs under surgery. Ten Cate (**45**) observed surgical analgesia in angelfish, Marcuse and Moore say: "On one occasion the experimenter inadvertently cut the animal with the scissors, drawing blood, but elicited no reaction" (**22**, p. 237). Coriat, however, says that "In neither the frog nor the guinea pig were there any signs of cutaneous anesthesia during the cataleptic states" (**6**, p. 344).

Steiniger (**40**), in accordance with his concept of unimpaired sensation during immobilization, believes that pain is felt by the immobilized animal but is not overtly manifested because of its inability to move.

General reactivity. In general, animals are said to be insensitive to stimulation, although diversity of opinion exists. Holmes (**17**), describing Faria's beetle experiments, points out that the presence or absence of moving objects or sound did not affect immobility. In his own experiments with water scorpions, Holmes (**18**) found his subjects responsive to light, contact, moisture and sudden changes in temperature. Rabaud (**32**) implies decreased sensitivity when he says that contracted muscle (the immobilized state producing tonic contraction) is less excitable. Beritoff (**1**) claims there is a decrease in reactivity; however, he qualifies this by pointing out that the immobile state is composed of two phases, active and inactive, and only in the first, active phase, is decreased reactivity found. Stroder (**43**) writes that all reactions are absent during immobilization.

Bonnet and Saboul (**3**) found increased electrical stimulation neces-

sary to elicit reactions from a frog. Foley (**10**) cites the "lack of overt reactivity to external stimulation" as another characteristic of the state, and, in defining the condition, calls it "negative or quiescent behavior, even in the presence of disturbing stimulation." Hoagland includes lack of reactivity to environmental stimuli as a characteristic of the state, although he adds that in lizards "the mere movement of objects in the environment was often sufficient to bring about a premature recovery" (**15**, p. 718). Rode (**37**) observed that, though the presentation of auditory or visual stimuli was ineffective, the state could be instantly terminated in the owl by touching its extremities. Reisinger (**33**), working with crayfish, claimed that light or darkness, and electricity were inconsequential in their effects on immobilized subjects. Bramwell (**4**), Verworn (**49**), and Steiniger (**40**) believe that sensation of the stimulus in an immobilized state is not diminished, even though reactivity is inhibited.[1]

Depth of trance. In relation to the study of reactive behavior the question of the depth of the trance has received scant attention. At best, Beritoff (**1**) merely indicates that the condition is not always at the same level from the moment of inception to the moment of cessation. His opinion, however, is without experimental support, while other investigators, apparently considering the state uniform in depth at all times, likewise show no evidence to support the contrary opinion.

Susceptibility. The problem of susceptibility as against non-susceptibility has not received very extensive attention. Both Hoagland (**14**) and Rabaud (**32**) believe that tonic immobility is of an all-or-none character. Coriat mentions that susceptibility in any one subject is variable: "Sometimes a given experiment would succeed with one animal, on other occasions the experiment would be a failure with the same animal" (**6**, p. 344). Darwin (**8**), writing of prolonged duration in some species, mentions that other varieties will not "feign" twice in succession. Holmes (**17**) mentioned the appearance of nonsusceptibility in scorpions at different times. McGonigal (**21**) mentions individual differences in susceptibility among the insect species. Dearborn observed that: "Susceptibility seems to increase distinctly with repetition of the impression on the average, but this is very erratic" (**9**, p. 424). He, too, notes, variation in reaction from subject to subject.

Several investigators have observed the influence of specific factors on susceptibility. Holmes (**18**) mentioned that induction of the trance

[1] The concept of "reactivity" needs clarification. Subjects may react, in the eyes of an experimenter, because they make gross, clearly perceptible movements. On the other hand, the subjects may make movements so minimal that they are apt to go unnoticed (eye blinking, minute attempts at limb flexion, low vocalization) or else, if observed, are considered only partial movements, and thus may or may not be considered a reaction. Motor reactions, then, may be total (spontaneous rise), partial (head or limb movements) minimal, or completely lacking.

when the subjects were under water (the natural habitat of water scorpions) was very difficult. Hoagland (**13**) claimed that amyl nitrate prohibited the appearance of the state.

Some investigators have attempted to determine the relationship of the mechanisms of balance to the induction of the trance condition (**15**, **50**). Extirpation of the vestibular centers, however, produced no difference in the susceptibility of animals to immobilization. Still others have attempted to find a relationship between cerebral ablations and the immobility reaction. Tonkikh (**47**), working with frogs, found no change when the hemispheres were removed. Verworn, describing the tonic contraction occurring in the state, says: "Dieser charakterische Symptomen-Complex tritt auch noch ein, wenn man den Thieren das Grosshirn herausgenommen hat" (**49**, p. 154). The possible effect of taming had received no attention until the work of Steiniger (**40**). He found that birds confined to cages in a laboratory were less suceptible than birds allowed to roam freely. Comparisons of free and "tame" animals were made on the basis of the tendency to spontaneous immobility, induced by the presence of human beings. This difference, he explains, is due to diminution of fear in the "tame" animals by virtue of their relatively greater familiarity with man. Gilman, Marcuse and Moore (**12**) found that "taming," *i.e.* experience with handling by humans, significantly decreases susceptibility in chickens.

Duration. Piéron, studying frogs, said: " . . . on assure son immobilité pendant un long temps qui . . . peut devenir de plus en plus long . . . qu'on répète l'expérience" (**29**, p. 230). Moll (**24**), who also studied frogs, claimed that duration increased with repetition of the experience.

Not all writers, however, believed that duration increased with repetition. Rabaud wrote: "Il ne me parait pas prouvé que cette durée s'acroisse ni qu'elle diminue avec les répétitions" (**31**, pp. 75–76). In Holmes' earliest paper (**16**), he indicates the diminution of trance duration with repetition, and, ten years later (**18**), wrote that all the forms studied showed a gradually decreasing duration as successive "feints" were induced. McGonigal (**21**) stated that graphs of duration all showed a sharp peak at the beginning, and then declined slowly. Mowrer (**25**) also noted decreased duration. Hoagland states: "The duration of the periods of immobility vary considerably from species to species, and to a lesser degree with individuals within the species" (**15**, p. 427). He describes the character of variation of duration as varying rhythmically in most cases. So strongly did he feel the rhythm to be apparent that he explained deviations from the rhythm as predictive of death.

Other writers merely note the duration periods in their respective subjects and mention the variability of the condition from time to time in a single subject, or between individual subjects. Rode (**37**) wrote of his owl that duration was never less than 15 minutes, and often better

than one hour. Volgyesi (50) also found repeated, prolonged duration in the owl. Minami and Dallenbach (**23**) write that the duration of inactivity in cockroaches is not long and varies considerably.

Several investigators have described the effect of particular factors on duration. Holmes (**18**) believed that the use of a dim light produced a longer "feint," while the trance could be shortened by the appearance of a bright light. Fabre, according to Holmes (**17**), found that extremes of heat or cold prolonged the trance in beetles. Reisinger (**33**), on the other hand, claimed that light and electricity did not affect the condition at all. Increased duration was obtained by Hoagland (**13**) with the administration of ergotamine and adrenalin. Rode (**37**) claimed that the state could be instantly terminated by touching the extremities of the owl on which he made his observations, but added that such provoked awakening was followed by a few seconds of torpor, particularly after a long immobilization. Dearborn (**9**) wrote that sensory stimulation precipitated the cessation of the trance. Removal of portions of the brains of water scorpions was said to decrease duration (**17**).

Some writers have attempted to explain the decrease in duration as a function of fatigue. Holmes said " . . . one would expect to find a diminution of the duration, and perfection of the response as a simple consequence of fatigue" (**19**, p. 212). Rabaud, who disputed increase or decrease with repetition, made some allowance for fatigue when he admitted: "Peut-être, cependant, intervient-il à la longue un phénomène de fatigue tel que les excitations ne parviennent plus à immobiliser l'animal" (**31**, p. 76). But the number of repetitions are not cited in the data, nor is one informed whether repetitions are immediately successive or extend over a period of days. Gilman, Marcuse and Moore (**12**) report a decrease in trance duration when immobilization trials extend over a 21 day period. When the same number of trials are given in a one day period no such decrease is found. These results are said to argue against an explanation in terms of fatigue.

Phylogenetic distribution. In Volgyesi's survey (**50**) of the history of "animal hypnosis," he mentions a sufficiently varied group of animal subjects to indicate the general phylogenetic spread of the phenomenon. Frolov also states " . . . the capacity of becoming immobile under the influence of compulsory restriction of movement is characteristic of all species and ages" (**11**, p. 158). The literature deals with the application of immobilization techniques to birds (**2**, **12**, **41**, **50**, **51**) reptiles such as the lizard (**14**), amphibians (crabs) (**33**), frog (**54**), octopus (**46**), insects such as water scorpions (**16**), cockroach (**23**), and a variety of mammals that includes the pig (**22**), monkey (**10**), dog (**28**), lion (**50**), and man (**53**).

Some writers were of the opinion that the state reached its maximum in the insect group (**7**, **17**). Coriat felt that

. . . the lower in the phylogenetic state was the animal experimented upon, the

easier it became to produce a motionless state. Furthermore, the duration of the state in the crustacean was longer than in those vertebrates which had a fairly differentiated nervous system. (6, pp. 344–345).

If, however, duration of the state is the criterion, such a belief is far from tenable since extremely long periods of duration appear in many groups higher in the phylogenic scale. It would be safest to rest with the knowledge of the rather consistent appearance of the condition all along the biological scale.

It is of interest to note that an analogous condition has occasionally been observed in the human being. The condition known as cataplexy takes the form of cataleptic fits which are elicitable by sudden emotion (fear, laughter). Wilson, presenting some illustrative case histories, concludes that it may be the perpetuation of " . . . a biologically old automatism, a defense-reaction in which immobility is of advantage to the frightened animal" (53, p. 51).

Hoagland makes another observation on analogous behavior in humans. He writes:

I have been able to produce the condition in adult human beings. If one bends forward from the waist through an angle of 90 degrees . . . and . . . is violently thrown backwards . . . the skeletal muscles contract vigorously and a state of pronounced immobility . . . may result (14, p. 716).

"Explanations" of the Phenomenon

Reflex response and cerebral inhibition. Some writers have attempted to explain the state by dealing with what was believed to be the neural mechanism producing it. Rabaud says it is ". . . une proprieté fondamentale du systeme nerveux et dérive, comme telle, de la constitution des organismes considerés; elle en est l'une des résultantes et ne saurait avoir d'autre raison d'être" (32, p. 141). He goes on to explain that the alternating mobilization-immobilization impulses of the nervous system produce motion and rest, respectively, and thus the condition would arise. He sums up its biological significance by saying: ". . . l'immobilisation réflexe est un cas particuler, une simple exagération du tonus musculaire normal" (32, p. 128).

Bonnet and Saboul (3) attribute the condition to a depression of reflexes caused by the modified function of the medullary centers. Verworn described the mechanism as an irritability of the cerebrum and inhibition of the motor areas of the brain. Introducing the idea of the "Lagereflex," he wrote: "Sie ist nichts anders als die Hemmung von spontanen Bewegungen oder Handlungen . . . wenn gleichzeitig ein starker Sinneseindruck uns intensiv erregt. . . . Das Wesentliche der Erscheinung ist vielmehr der tonische Lagereflex" (49, p. 54).

Coriat speaks of "cerebral inhibition," but adds, "I cannot agree

with the hypothesis that cerebral inhibition is a reduction of psychic intensity. This inhibition of the brain probably took place through the kinesthetic stimuli of the manipulations being poured into the central nervous system. . . . " (6, p. 346.)

Rijlant (35) believed the condition to be a result of the modified state of the central nervous system which produced a state of muscle contraction. Pavlov (28) and Frolov (11) used neural inhibition as their explanation for the phenomenon. Ten Cate (45) emphasized permanent excitation as the causative agent. Holmes (17), although using the term "death feint," rejects the explanation implicit in the term, and considers immobility to be some kind of "thigmotatic response." Verworn's concept (49) of "position-correction" is likewise reflexive.

Hoagland postulates the existence of inhibitory hormones acting on the central nervous system: "These hormones are assumed to produce tonic immobility, when present above threshold concentrations, by inhibiting impulses from 'higher nervous centers' and by allowing impulses to pass from the tonic centers to the muscles" (13, p. 842).

Death feigning. The theory of death feigning, or the simulation of death, posits the idea that the animal, in the face of danger, instinctively succumbs to immobility as a protective guise. Darwin (8) was one of the first to speak of this idea, although he notes that "fainting" with the paralyzing effects of fear may be confused with the instinctive death-feint reaction. Some doubt may have entered his thinking, for he noted, in a comparison of dead and "feigning" insects, that the immobilized insects had not adopted the characteristic death posture. Bramwell wrote:

> I am inclined to think that in many instances it is a conscious simulation of death, adopted by the animals from the instinctive knowledge of the fact that certain birds and beasts of prey, except under pressure of extreme hunger, will not attack what is dead . . . death, or catalepsy, was only shammed . . . (4, p. 156).

Criticism can be raised as to the possibility of such voluntary behavior being within the intellectual range of subhuman species.

McGonigal raises a question on the teleological value of the death feint.

> The interesting point of this whole phenomenon is that, assuming with Darwin that this process was developed as a protective instinct to frighten away enemies (a presumption not as yet superseded), in this case teleology has outdone itself, for a passionate or excitable individual often rises so vigorously and on limbs so rigid that he falls on his back. . . . (in) the power of any foe . . . comparatively helpless and exposed (21, p. 408).

This criticism would seem to be an important one, for just as one would posit an instinct to feign death, one would have to assume that animals of prey had an instinct to avoid death-feigning, or dead, animals.

Sleep. Several people have adopted the theory that the condition is sleep, or some form of it. Heubel and Wundt (**24**), Sidis (**39**) and Pavlov (**28**) support this hypothesis. Heubel and Wundt considered the resting conditions (sleep and immobility) to be the consequence of the cessation of external stimulation. Sidis, however, considered this a "hypnoidal" or sub-waking condition and not true sleep.

Hypnosis. Although the term "hypnosis" has been used by some investigators, it has customarily been used with the qualification, "so-called." Erhard (**30**) has supported this theory, but the most vigorous exponent of hypnosis is Volgyesi (**50**) who criticized sharply those who used the term, "so-called animal hypnosis." He strongly believed the condition to be the result of hypnotic influence. Bramwell, however, had earlier indicated his opposition to the hypnosis theory, writing:

> The more one appreciates the complexities of the hypnotic state—the fact that it is essentially distinguished by an increased power of controlling the organism, without diminished consciousness or volition—the more one must recognize that it finds no analogy in an animal deprived of its cerebral hemispheres, and discharging a continuous stream of nervous motor impulses from its lower centres (4, p. 157).

Spatial disorientation. Verworn (**49**), Rabaud (**31**), and Hoagland (**14**) have attempted to describe immobility as primarily related to an impairment of the customary spatial relationship of an animal and its environment.

Paralysis of fear. The principle idea presented in this hypothesis is that of a paralysis produced by fear. Preyer (**30**) first emphasized the fear factor by speaking of "cataplexy." McGonigal (**21**) implies the presence of fear, when he points out that Holmes' successful attempts at immobilizing water scorpions might not have occurred if the subjects had been permitted to remain in their natural habitat while being immobilized. Rujka (**38**) extends the fear explanation further by considering the state to be a form of anxiety reaction. Steiniger (**40**) believes the state of inhibited reaction to be attributable to two factors: the psychological factor of fear which induces the condition, and the instinctive reflexive basis for the phenomenon. Its ecological importance, he says may have two explanations. Vertebrates in this condition can, in an instance of approach or pursuit by an enemy, be overlooked by the absence of any movement on the part of the immobilized animal.[2]

[2] Although Steiniger specifically rejects any notion of volitional behavior, his explanation seems to leave room for just such an implication. For the captured animal to be

Gilman, Marcuse and Moore (12) emphasize the importance of fear as a factor determining not only susceptibility but also duration of trance in chickens. They suggest that fear and disrupted stimulus-reception are interactive in the sense that interference with normal proprioception may evoke fearful behavior and thus both factors may be operative in eliciting the experimentally induced immobile state.

Summary

It has been found that under certain conditions a "trance" state can be induced in a wide variety of animals. Attempts to picture the characteristics of this condition have, as a result of inconsistencies in technique, been discrepant in their conclusions. A variety of methods for deliberately inducing the condition have been referred to in the literature, but all seem to involve, to some degree the factor of restraint of movement. Since its description in 1646, the principal hypotheses have been death feigning, hypnosis, spatial disorientation and paralysis of fear.

BIBLIOGRAPHY

1. Beritoff, J. Ueber die Enstehung der tierischen Hypnose. *Z. Biol.*, 1929, **89**, 77–82.
2. Boas, E. P., & Landauer, W. The effect of elevated metabolism on the hearts of the frizzle fowl. *Amer. J. med. Sci.*, 1933, **185**, 654–664.
3. Bonnet, V., & Saboul, R. Contribution a l'étude de l'hypnose animale. *J. Physiol. et Pathol. generale*, 1935, **33**, 887–906.
4. Bramwell, J. M. *Hypnotism.* London: DeLaMare Press, 1906.
5. Carrington, H. Hypnotizing animals. *Psychic. Res.*, 1931, **25**, 41–44.
6. Coriat, I. H. The nature of sleep. *J. abnorm. Psychol.*, 1912, **6**, 329–367.
7. Crozier, W. J. Reflex immobility and the central nervous system. *Proc. Soc. exp. Biol.*, 1923, **21**, 55–56.
8. Darwin, C. A posthumous essay on instinct. In G. J. Romanes, *Mental evolution in animals* (pp. 360–364). New York: Appleton, 1900.
9. Dearborn, G. V. N. Psychophysiology of the crayfish. *Amer. J. Physiol.*, 1900, **3**, 404–443.
10. Foley, J. P., Jr. Tonic immobility in the rhesus monkey (Macaca mulatta) induced by manipulation, immobilization and experimental inversion of the visual field. *J. comp. Psychol.*, 1938, **26**, 515–526.
11. Frolov, Y. P. *Pavlov and his school.* London: K. Paul, Trench, Trubner, 1937.
12. Gilman, T. T., Marcuse, F. L., & Moore, A. U. "Animal hypnosis": A study in the induction of tonic immobility in chickens, *J. comp. and physiol. Psychol.* (In press.)
13. Hoagland, H. Quantitative aspects

able to escape at a convenient time, it would have to be able to recognize the opportunity when it came, and be able to mobilize its muscular resources for escape. When the effects of immobilization wear off, animals frequently spring to an upright position, but there is no evidence that the immobilization disappears when the chance for flight arises.

of tonic immobility in vertebrates. *Proc. Nat. Acad. Sci., Wash.*, 1927, **13**, 838–843.

14. Hoagland, H. On the mechanism of tonic immobility in vertebrates. *J. gen. Physiol.*, 1928, **11**, 715–741.
15. Hoagland, H. The mechanism of tonic immobility, *J. gen. Psychol.*, 1928, **1**, 426–447.
16. Holmes, S. J. Death-feigning in Ranatra. *J. comp. Neurol. Psychol.*, 1906, **16**, 200–216.
17. Holmes, S. J. The instinct of feigning death. *Pop. Sci. Mon.*, 1908, **72**, 179–185.
18. Holmes, S. J. *Studies in animal behavior*. Boston: Badger, 1916.
19. Holmes, S. J. A note on tonic immobility. *J. gen. Psychol.*, 1928, **2**, 378.
20. Lush, J. L. "Nervous" goats. *J. Hered.*, 1930, **21**, 243–247.
21. McGonigal, J. P. Immobility: an inquiry into the mechanism of fear reaction. *J. abnorm. Psychol.*, 1920, **27**, 73–80.
22. Marcuse, F. L., & Moore, A. U. Tantrum behavior in the pig. *J. comp. Psychol.*, 1944, **37**, 235–241.
23. Minami, H., & Dallenbach, K. M. The effect of activity upon learning and retention in the cockroach. *Amer. J. Psychol.*, 1946, **59**, 1–58.
24. Moll, A. *Hypnotism*. London: Scott, 1910.
25. Mowrer, O. H. A note on the effect of repeated hypnotic stimulation, *J. abnorm. soc. Psychol.*, 1932, **27**, 60–62.
26. Newman, J. B. *Fascination or the philosophy of charming*. New York: Fowler Well, 1854.
27. Parker, G. H. The excretion of carbon dioxide by relaxed and contracted sea anemones. *J. gen. Physiol.*, 1922, **5**, 46–64.
28. Pavlov, I. P., & Petrova, M. K. A contribution to the physiology of the hypnotic state of dogs. *Character and Pers.*, 1934, **2**, 189–200.
29. Piéron, H. Le problème physiologique du sommeil. *Libraries de l'Académie de Médecine*, 1913, 230–235.
30. Preyer, W. *Die Entdeckung des Hypnotismus*. Berlin: Gebrüder Paetel, 1881.
31. Rabaud, E. Le phénomène de la "simulation de la mort," *C. R. Soc. Biol. Paris*, 1916, **79**, 74–77.
32. Rabaud, E. L'immobilisation réflexe et l'activité normale des arthropodes. *Bull. Biol.*, 1919, **53**, 1–149.
33. Reisinger, L. Hypnose des Flusskrebses. *Biol. Zbl.*, 1927, **47**, 722–726.
34. Reisinger, L. Katalepsie der indischen Stabheuschrecke, *Biol. Zbl.*, 1928, **48**, 162–167.
35. Rijlant, P. Étude chez la poule des activités toniques et contractile du muscle strié pendant l'hypnose. *C. R. Soc. Biol. Paris*, 1933, **113**, 417–421.
36. Rijlant, P. Le tonus musculaire chez un mammifère en état de l'hypnose. *C. R. Soc. Biol. Paris*, 1933, **113**, 421–424.
37. Rode, R. Observations sur le phénomène d'immobilisation réflexe chez la chouette chevêche, Athene noctua Scop. *Bull. Soc. Zool. France*, 1930, **55**, 451–454.
38. Rujka, T. Beiträge zu den biologischen Grundlagen des Zusammenhanges zwischen reflektorischer immobilization und Angstzuständen. *Riv. Biol.*, 1938, **26**, 317–342.
39. Sidis, B. An experimental study of sleep. *J. abnorm. Psychol.*, 1908, **3**, 1–32.
40. Steiniger, F. Die Biologie der sogenannte "tierischen Hypnose." *Ergebn. Biol.*, 1936, **13**, 348–451.
41. Steiniger, F. Der Eingluss der Zahmheit auf die sogennate "tierischen Hypnose" bei Vögeln. *Z. Tierpsychol.*, 1941, **4**, 260–271.

42. STEINIGER, F. Eine Erwiderung auf das Buch von Franz Volgyesi, Menschen- und Tierhypnose. *Z. Tierpsychol.*, 1941, **4**, 272–280.

43. STRODER, J. Ueber der Einfluss der "tierischen Hypnose" auf den Ablauf der Narkose am Kaltblüter. *Schmerz Narkose-Anaesth.*, 1938, **11**, 82–84.

44. SUTHERLAND, G. F., & CURTIS, Q. F. Myotonia Congenita in the goat. *Proc. Soc. exp. Biol., N. Y.*, 1938, **38**, 460.

45. TEN CATE, J. Sur la production de ce qu'on appelle l'état d'hypnose animale chez la raie. *Arch. néerl. Physiol.*, 1928, **12**, 188–190.

46. TEN CATE, J. Nouvelles observations sur l'hypnose dite animale. État d'hypnose chez octupus vulgaris. *Arch. néerl. Physiol.*, 1928, **13**, 402–406.

47. TONKIKH, A. V. The role of the autonomic nervous system in the phenomenon of the so-called "animal hypnosis." *Fisiol. Zh. S.S.S.R.*, 1938, **24**, 367–371.

48. VERGER, M. P., & LAFON, M. J. La cataplexie. *Encéphale*, 1939, **34**, 121–148.

49. VERWORN, M. Die sogenannte Hypnose der Thiere. *J. Physiol.*, 1899, **23**, 53–54.

50. VOLGYESI, F. *Menschen- und Tierhypnose.* Leipzig: Fussli, 1938.

51. WARNKE, G. Zur Akinese bei jungen Möwen und Störchen. *Zool. Anz.*, 1937, **118**, 17.

52. WILSON, J. *Trials of animal magnetism on the brute creation.* London: Sherwood, Gilbert and Piper, 1839.

53. WILSON, S. A. K. Cataplexy. *J. Neurol. Psychopath.*, 1933, **14**, 45–51.

54. YERKES, R. M. The instincts, habits and reactions of the frog. *Psychol. Rev.*, 1903, **1**, 579–597.

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BOOK REVIEWS

Evans, R. M. *Introduction to color.* New York: Wiley, 1948. Pp. x+327.

Once in a blue moon a book appears which brings together many important developments in a field at a time when integration is imperatively needed. The subject of color embraces a vast domain, ranging from physics to psychology on the scientific side and from industry to art on the applied side. The author of this book, who is Head of the Color Control Department of Eastman Kodak Company, is well equipped for his task with a background of training in optics and photography, with industrial experience in still and motion picture photography, and with an active interest in what the psychologist has to offer in unravelling the riddles of color. As a result this book deals not only with basic physical considerations but also includes a good deal of phenomenological material. Applications to color photography, art, and design are not overlooked. Two of the most difficult aspects of the subject for the lay reader, mathematics and a highly technical nomenclature, are adroitly side-stepped without any sacrifice in clarity and accuracy of exposition. All in all this is a notable contribution to visual science.

It is not possible within the confines of this review to give an adequate picture of the careful, detailed discussion of fundamentals and the vast amount of information this work contains. Anyone working seriously in the field of color today must understand how visual stimuli are measured and specified, either in terms of the I.C.I. standard observer or by means of an equivalent system; he must be aware of and on guard against errors inherent in photometric, spectrophotometric, and colorimetric measurements, especially with respect to the newer fluorescent materials coming into use; and he must have some knowledge of the vagaries of light, paint, and pigment mixtures. Certain phenomena, like dichroism and metamerism, if not properly understood, can be the source of faulty inferences regarding both the nature of the stimulus and the behavior of the eye. These and many other matters concerning light sources and reflecting, transmitting, and mixture characteristics of solid, liquid, and colorant substances are fully discussed and illustrated by means of excellent diagrams and superb colored plates.

The power of a quantitative treatment of color comes out particularly in the many uses to which the I.C.I. mixture diagram can be put. Not only does it provide a means of specifying dominant wave-length and purity of stimuli and the results of mixture of any number of lights, but it also shows changes in chromaticness of objects in different illuminants and of mixtures of dyes, paints, or pigments in various amounts or concentrations. Maximum purity theoretically obtainable from sur-

faces of any reflectance are thereby easily and accurately diagrammed. In spite of its limitations this method of specification of stimuli should be more widely known and used in psychology.

Along with detailed handling of the physical conditions under which colors are seen will also be found a wealth of phenomenological observations intimately related to physical and physiological antecedents. The discussion is not limited to color in the narrower sense, for matters like the perception of depth, the texture of surfaces, and the nature of shadows are explained by reference to position and size of light source, the reflecting properties of objects, and the like. It is in this area, the meeting place of psychology and physics, that this reviewer believes psychologists will find the book of greatest interest and value even though the author's concept of psychology does not seem wholly satisfactory. Let us turn to this question since it is basic to most of the work being done in the field of color whether by psychologists or others.

The key to Evans' treatment of color as most psychologists understand the concept is found in the position adopted in the 1943–44 Report of the Colorimetry Committee of the Optical Society of America wherein the subject is divided into three parts: (1) physics, which deals with sources and modifiers of light energy; (2) psychophysics, which deals with the evaluation of radiant energy by the eye under a single, specified set of conditions, viz., matching small foveal stimuli against a completely dark background; and (3) psychology, which deals with factors like the modes of appearance of Katz, so-called perceptual attributes of vision like texture, size, location, flicker, lustre and glossiness, and other factors like attention, feelings, attitudes, and intentions as they affect colors. Psychology is regarded as having to do only with subjective, qualitative matters. This position will come as a shock to those who believe in and work in psychology as an experimental, quantitative science.

At first sight the three-fold division of color seems as if it might offer a good way out of many of the difficulties which beset workers in the field of visual science. But as the realm of physics is left behind and the organism enters more and more into the total process, the split between psychophysics and psychology seems arbitrary and confusing. The mental keeps popping into the exposition, upsetting the scientific applecart again and again. At times the author himself is not sure of the validity of his fundamental position, *e.g.*, when he says: "This difference (in the way of looking at the stimulus) suggests that what has been described as a difference in attitude may, in the last analysis, be found to be a difference primarily in the way the eyes are used rather than in the mental phase. A consequent decision that the effect is purely psychophysical may be involved" (p. 166). To the psychologist it is obvious that the definition of psychophysics adopted by the Colorimetry Committee is too narrow and leads to insuperable difficulties for any con-

sideration of color based on it. Since the time of Fechner a number of psychophysical methods other than the null method of the standard observer have been found to yield statistically reliable results in studies of vision, hearing, kinaesthesis, taste, and smell. That some of the difficult problems of colorimetry, *e.g.*, the problem of color differences, and so-called color appearances, will not be solved by taking results from the method of just noticeable differences or the null method of gauging the spectrum, and forcing them to yield information about equal sense distances or equal ratios, must be clear to all by now. A definition of psychophysics that includes the procedures made possible by the work of such psychologists as Urban, S. S. Stevens, Guilford, and certain contributions of Thurstone and others might prove very useful in approaching color problems now relegated to the psyche.

On the other hand, due weight is given in this book to factors like adaptation level and contrast in explaining color phenomena which many psychologists have been content to label "constancy" and let go at that. Part of the reason for abandoning these concepts when dealing with phenomena of constancy and color conversion may be due to the fact they were conceptualized too simply. As Evans points out, there are at least three types of adaptation: general, local, and lateral (with temporal effects to complicate each, it may be added); and three types of contrast: subject, object, and illumination contrast. Whether or not these types of adaptation and contrast are merely descriptive or are fundamentally different is still an open question.

Many psychologists will miss consideration of color theories and a good deal of information which physiology supplies in connection with this topic. Evans seems to find the tri-receptor theory plus a brightness receptor and a new concept, "cross talk" among nerve fibers, adequate for explaining a number of crucial phenomena. Until an alternative theory to the Young-Helmholtz theory comes along which is equally simple conceptually and as convenient for quantitative work we may expect people to go on using the older formulation in spite of its known shortcomings.

To sum up: this book constitutes a very important contribution to visual science and will exert considerable influence for a number of reasons. First, it makes available a great deal of material scattered in scientific, technical, and trade publications. Second, it relates a large number of phenomenological data to their antecedent physical conditions. Third, it opens up many possibilities for further investigation to the discerning reader. That the psychologist can play an important role in the future of visual science no one can doubt after reading this book, because it shows that many of the problems are predominantly psychological in character. Progress will come through the application of quantitative methods to color phenomena now relegated to mere "appearance," or to subjective factors like intentions or illusions. Evans

has brought visual science farther along the way by showing that a good deal that used to be thought to be "purely psychological" is amenable to objective treatment. Much still remains to be done, however. If we may regard this book as a contribution by a physicist with a strong psychological slant, then we may hazard the opinion that there is room for another book by a psychologist with a strong physical slant!

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DOOB, LEONARD W. *Public opinion and propaganda.* New York: Holt, 1948, $3.75.

Teachers of courses in the psychology of public opinion and propaganda surely need a good textbook. If Doob's new text does not fulfill this need completely, it is nonetheless—and by far—the best text to be had.

A word first about its point of view and organization. The author takes as his systematic theme, announced in the opening chapters, the behavior theory of Clark Hull and the Yale School. With varying success, the chapters which follow attempt to apply Hullian principles to an understanding of attitude formation and maintenance, the development of public opinion, the principles of propaganda, and the myriad of behaviors which constitute mass communication. There are twenty-one chapters covering such topics as the cultural background of public opinion, sampling of opinion, the techniques of polling and intensive surveying, the nature of propaganda, content analysis, the perception of propaganda, the relation of learning and personality to propaganda reaction, and a triad on press, radio, and "the media of sight and sound."

Evaluation of Doob's book must proceed on two levels. In the first place, it is more than a textbook, representing as it does one of the few systematic attempts to apply a consistent theory of behavior to the vast realms of opinion and communication. And then, it is also a textbook.

Consider Doob's book first as an attempt at systematization. In the opening chapter, Doob urges that, in the study of opinion and propaganda, we deal not with aggregate entities, but rather with the principles of individual behavior. Let us, he says, talk not of culture or society or social pressures in the abstract, but in terms of their impact on individual behavior. The necessary equipment for such an analysis of individual behavior, Doob feels, can be stated in terms of these basic principles: the S-R relationship; the concept of drive as a tension state; the law of effect stated in terms of tension-reduction; the intervening variable of personality which comprises the organization of tendencies, which mediate between S and R; the concept "attitude," of which more,

shortly; the Hullian doctrine of habit and habit organization; and finally knowledge, variously denoted as know-how or skills or subjective, cognitive context. Attitude, obviously the central term, is defined in simple terms as "the socially significant internal response that people habitually make to stimuli." "Public opinion refers to people's attitudes on an issue when they are members of the same social group." That, in essence, is the conceptual core of Doob's approach, and to it he sticks with striking tenacity through chapters ranging all the way from the learning of propaganda to the printed media.

In Chapter 5, a crucial one dealing with the "behavior of public opinion," we see a rather crucial test of the adequacy of 190 proof Hull as applied to public opinion. The result is curious. One can go only so far, in the kind of theorizing which accounts for the development of internal responses (attitudes) in terms of learned drive-reducing behaviors, of successful propaganda as drive-reducing stimuli, and so on. When Doob comes down to an attempt to state some laws about public opinion, however, we discover that he too is strapped, for, lo and behold, there appears before us a description of opinion formation mechanisms in terms of the classical mechanisms of ego-defense: rationalization, displacement, compensation, projection, etc. The chapter concludes with some stunningly thought-provoking general laws of public opinion which attempt to relate the formation and expression of opinion to such phenomena as frustration, anxiety, and the need for conformity. For the reviewer, the chapter appeared as the theoretical high point of the book, the one point at which he felt liberated from the interminable circle of things working because a habit existed, because a stimulus had reinforcing value, or because a bit of behavior brought about a reduction in drive state directly or indirectly. The theoretical model, rigorous though it may be, and useful though it has proved in certain restricted forms of behavior analysis, is a woefully impoverished one when placed alongside the real thing of public opinion and propaganda. While there are some conceptually provocative sections, one does not feel that Doob has succeeded in doing theoretical justice to the richness of the material which he treats on the descriptive level. For at this level, Leonard Doob is a knowledgeable, shrewd, and amusing observer who sees and understands much.

We must talk now about Doob's work as a textbook. It is a good textbook, and it is packed with a great deal of recent material on problems in opinion and communication. A review of this sort cannot cover all of this material and its organization in the book. Let us merely try to touch on a few of the high points, some low points, and then try to draw up a balance. The chapters on the sampling of opinion and the mechanics of polling are excellent, better than anything available for large class use. They are informative, full, and direct in exposition. So

too, the chapter on the importance of public opinion, which represents a thoughtful departure from the arrant romantic nonsense which makes of public opinion an infallible demigod. The sections on propaganda have an admirable internal organization which takes the reader through propaganda from its perception, through the role of personality in mediating response, through the learning of response to propaganda, to propaganda-induced action. The point of view represents, however, little change from Doob's earlier book on propaganda. Its organization, however, is much tighter.

Now the low points. The chapters on the printed media and on radio are more impoverished than they need be. There is insufficient attention to studies of readership of newspapers, and the treatment of book reading has no reference whatever to the major studies in the field: the work of Waples and the national survey of Link and Hopf. The chapter on radio is good on statistical description of listening, but fails to take advantage of the rich literature dealing with gratification studies, the "why" of radio listening. In place of reviewing these, Doob gives a rather spotty presentation of his own propaganda principles applied at random.

But the biggest failure of the book as a textbook is its failure to present adequately, and in context, the major studies which have made of public opinion research something more than a collation of newspaper clippings from the Gallup Poll. The chapter entitled "Evaluation of Polling" is an excellent critique of some of the adolescent extravaganzas of the pollsters—in which, unfortunately, various people are pilloried for views they never thought they had—but this good criticism is not counterbalanced by the presentation of more basic and constructive work. Why not some extended treatment of such monographic studies as Lazarsfeld, Berelson, and Gaudet's *The People's Choice;* of Jones' *Life, Liberty, and Property;* of Merton's *Mass Persuasion;* of Murray and Morgan's *Clinical Study of Sentiments;* of Newcomb's *Personality and the Social Pattern;* of Likert's Federal Reserve studies; of Link and Hopf's *People and Books*? And why so little on attitude scaling? Doob seems to preoccupied at times with his conceptual task that he has tended to forget the task of presenting to his student readers a full picture of the substantive content of the field of public opinion and propaganda.

The balance, unfortunately, is not as high on the plus side as one might have expected in a work by so talented an analyst as Doob. We still lack an ideal textbook. But, for all its shortcomings, Doob's *Public Opinion and Propaganda* is clearly above the level of previous textbook attempts in this field.

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SHERIF, M. *An outline of social psychology.* New York: Harper, 1948. Pp. xv+479.

This text is intended as an introduction to social psychology and presents Sherif's approach to the subject as developed in his research and as expressed in his earlier writings, in particular, *A Study of Some Social Factors in Perception, The Psychology of Social Norms*, and (with H. Cantril) *The Psychology of Ego-Involvements.* The book is evidently meant for students who have had a course in general psychology, since it assumes considerable knowledge of psychological terminology and theory.

Sherif defines social psychology as dealing with "the experience and behavior of the individual in relation to social stimulus situations" (p. 1), namely "interpersonal relationships, group interactions and their products." These products include "values or norms, language, art forms, institutions and technology." For Sherif, the impact of social stimulus situations on the individual constitutes the process of socialization—"the central theme of social psychology."

Throughout the text, attention is paid to two chief determiners of the individual's perceptions, thoughts and behavior: his *motives* and his *reference groups.* The *norms* of his reference groups, past and present, have largely determined his *frames of reference* comprising his *ego-attitudes* (or *values*). However, at any given time, his *deprivations* (of *biogenic* and *sociogenic motives*) are critical selective factors in his perceptions, thoughts and behavior.

Sherif holds to the view that social psychology uses the same concepts as general psychology but "extends" them to the social field. Accordingly, he most frequently introduces topics by reviewing relevant analyses and research findings of general psychology, then relates the concepts thus explored to social contexts.

In the main, social psychologists have concerned themselves with three major themes: *personality structure and functioning* (apart from personality development), *socialization* (i.e., personality development) and *group dynamics* (the interaction of individuals in face-to-face groups). Sherif places almost exclusive emphasis on socialization, largely ignoring the other two themes. Nowhere does he venture a unified account of personality structure, though he uses such structural concepts as motive, perception, attitude, ego and ego attitude. He approaches a unifying concept most closely when he defines the ego as a "constellation of attitudes in the psychological make-up of the *human* adult" (p. 252). The nature of this constellation he does not specify. Likewise, Sherif makes no attempt to describe or explain how face-to-face groups are formed, or how individuals interact in such groups. This is not to say that he avoids mentioning such groups. Indeed, he talks about crowds, in-groups and out-groups, etc., and reports some of the research studies involving different sorts of groups.

The point is that he does not go beyond a very unsystematic treatment of group structures and processes.

In his discussion of socialization, Sherif emphasizes two things: the part played by individual factors such as food deprivation in determining social perceptions and attitudes, and the effects of group norms on individuals. His account of socialization is more descriptive than explanatory. In the main, he contents himself with showing that ego involvement occurs through social experience, and that group norms and individual ego attitudes are correlated. He brushes aside such approaches to explaining personality development as learning theory and Freudian theory. While he obviously inclines toward Gestalt "field theory," he does not discuss it systematically.

Sherif cites quite a number of research studies at length. He includes detailed accounts of the University of Minnesota starvation experiments, Newcomb's Bennington study (a section written by Newcomb himself), W. F. Whyte's study of street-corner gangs, and the leadership studies of Lewin and his associates. He devotes a good deal of space to his own experiments on autokinesis and ego-involvement.

The volume is quite uneven both in style and level of difficulty. Some of the passages seem to be directed toward the professional psychologist rather than the undergraduate student. Many of the concepts used throughout the book are not carefully defined. Sherif quotes from authorities (notably C. T. Morgan, A. Gesell and E. C. Tolman) more frequently than seems necessary in his discussion of the biological bases of social behavior. On the favorable side, Sherif makes effective use of novel and highly interesting illustrative material, much of it in the form of anecdotes relating experiences of participants in World War II.

There is virtually nothing in this book on the methods of social psychology. Even while describing experiments in detail, it is the results, not the methods, which Sherif stresses.

Certainly this is not the basic textbook in social psychology which we have all been waiting for, or trying to write. However, Sherif's approach to the subject is original and provocative, and his research contributions substantial. Many instructors who admire Sherif's thinking and research on frames of reference will welcome this volume since it makes his views more accessible to the undergraduate student than his earlier writings.

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HALSTEAD, WARD C. *Brain and intelligence: A quantitative study of the frontal lobes.* Chicago: Univ. Chicago Press, 1947. Pp. 206. $6.00.

Within the so-called modern era of scientific inquiry there have been many attempts to find physical and physiological correlates of intellec-

tual functions. On the physical side, these range from the pseudo-scientific approaches of Gall and Spurzheim, early in the nineteenth century, to the more refined anthropometric studies of recent date, some of which have culminated in general typologies. Among the physiological correlates sought may be mentioned reaction time, reflex time, chronaxie, metabolism, brain waves and others. None, as a separate measure, has proved effective or convincing. Yet, somewhere within the complex matrix of the living organism must lie the substratum upon which intelligent behavior depends for its development. In phylogenetic retrospect, the highest level of the central nervous system has seemed to many the most likely place to look because of its progressive hierarchy of integrative functions.

This is precisely what Halstead has done. Exercising courage, conviction and patience he has during the past ten years or so laid the ground-work for an entirely new conception of intelligence. In ***Brain and Intelligence*** he tells, albeit somewhat dramatically, how he has been able to utilize neurosurgical and brain-injured patients to tease out factors in a "biological intelligence" by some unique and some traditional test procedures. Halstead is a pioneer in a very important field of endeavor and he has made substantial progress in opening up this area of investigation. He has coupled quantitative, experimental methods derived from psychology and physiology with a judicious clinico-pathological insight developed through a period of concentrated work with neurological and psychiatric patients.

In view of Halstead's extensive and significant work in forging a pathway in a virtual wilderness, this book is somewhat of a disappointment. It is stimulating and provocative, but it is disappointing because it does not make clear the nature and significance of the basic concept which underlies the work, namely that a "biological intelligence" exists. Although it is evident that a great deal of careful and ingenious experimenting has been done, the book is clearly deficient in the portrayal of the experimental procedures and in the presentation of the experimental results. On the other hand, confusing hypothetical issues are given undue space. For example, there is a persistent effort to make the facts fit certain psychoanalytic concepts; this effort is obviously strained and without proportionate success.

Part I of the book is concerned with the structure of biological intelligence. It begins with an ambition to define and measure biological intelligence. Although the desire to be clear and straightforward about biological intelligence still persists in Chapter VI with the statement, "our objective in this chapter will be a definition of biological intelligence in operational terms," there is nowhere in the book, a clear statement, operational or otherwise, as to what is meant by it. The first four chapters, or nearly one fifth of the textual part of the book, are required to identify Freud's ego with biological intelligence and to dispose lightly

of previous theories of intelligence, psychometric, clinical, and neurological.

Chapter V briefly discusses the background for the present study and the method of selecting control subjects. The principle of "blended controls" was settled upon. These consisted of 14 "normals," 10 military patients with mild psychoneurosis, and 6 miscellaneous subjects under some psychological or biological stress at time of examination. One may question this procedure, as well as the matter of basing the factor analysis upon 50 military head injury cases, although reportedly recovered from mild concussions.

There is needless confusion throughout the book in reference to the number of subjects and tests involved. A total of 237 patients and control subjects and 27 tests (or indicators as Halstead prefers to call them) are repeatedly referred to, but Appendix D presents raw data for only 30 "controls" and 50 neurosurgical patients based upon 10 tests. Likewise, the summary implies that the results are based upon 237 subjects and 27 tests.

As an example of the sparsity of experimental data, Chapter VI presents the quantitative results and is composed of four and one-half pages and two tables. One is a table of intercorrelations for 13 tests or indicators, and the other contains the factor loadings for these tests as determined by the Thurstone method. Two other factor analyses supplied by Holzinger are not presented. Although these reportedly gave four-factor descriptions, the reader is not told how they agree with the four-factor solutions by the Thurstone method.

Chapters VII to X are devoted to a description and explanation of the four factors, whereas Chapter XI fits them into the nuclear structure of the ego. The four factors are: (1) a central integrative field factor (C), described as the organized experience of the individual; (2) an abstraction factor (A) found in grouping to categories and in understanding similarities and differences; (3) a power factor (P), a dynamic, energizing factor, said to counterbalance or regulate affective forces and permit further ego differentiation; and (4) a directional factor (D), which specifies the sensory modality of experience and the motor outlets—an exteriorization of biological intelligence.

Part II deals with the representation of the basic factors in the brain, the way these factors may be modified by experimental anoxia, and the differential impairment of the factors with different brain lesions. The latter aspect has been quantified in the form of an *impairment index*. The impairment index consists of a ten-point scale, one point being allotted for reaching a set criterion on each test. The score range is 0.0 to 1.0. Control subjects have a low index, whereas patients with frontal lobectomies have the highest indices. Frontal lobectomies show a higher impairment index than do equivalent ablations in other areas of the brain. It should be pointed out that only eight of the original 13

tests used in the factor analysis appear in the impairment index. Unfortunately, several errors are to be found in the impairment indices of tables 15 and 16. Also in Table 16 there are several instances of the calculation of an impairment index based on only three, four, or five of the test indicators out of a total of ten. This does not seem justified, particularly when different subjects might thus be compared on the basis of entirely different indicators. Appendices A and B provide, respectively, a brief history and diagrams of brain lesions for each of the 50 lobectomies.

Although the reader will find much to stimulate his thinking in this book, he will undoubtedly yearn for more adequate descriptions of the indicators and procedures for using them, for more data than are provided in the limited tables, and for more conclusive proof of the four factors and their significance and meaning. In general, he will probably wish that Halstead had described his tests and results completely, and perhaps had left ramifying theory and arm-chair speculation to be covered in a separate volume so labelled.

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